THURSDAY, AUGUST 23, 1894.

THE PHYSIOLOGY OF THE CARBOHYDRATES.

The Physiology of the Carbohydrates: their Application as Food, and Relation to Diabetes. By F. W. Pavy, M.D., F.R.S. (London: Churchill, 1894.)

AMONG the most debated subjects of physiology the ultimate fate of carbohydrates within the body stands out as one of the greatest difficulty. A great amount of work has been expended on it, but still many points of vital importance remain undecided. We accordingly hail with pleasure a work containing the results and conclusions of one whose name is indissolubly associated with the subject, as the author of one of the two rival theories around which much of the work of the last thirty years has centred. The book before us contains a revised description of the many researches of the author upon this subject, with a criticism of his earlier results by the light of his later work, and the greatly increased knowledge of the chemistry of the carbohydrates which is now available. Embodying as it does the results of a "life's labour, attended with unceasing laboratory work," we turn to it with more than ordinary interest.

The book is arranged in sections, in the first of which the author gives us a short account of the main chemical characters of the principal carbohydrates, and special stress is laid upon the production of osazones as enabling us to distinguish the different sugars from one another. This is followed by an account of the hydrations produced by ferment action and the changes known to occur through the agency of protoplasm. Succeeding sections deal almost entirely with Dr. Pavy's researches, arranged not in the order in which they appeared, but in such a manner as to gradually unfold the author's argument.

Thus we find described the experiments upon which he concludes that many proteids have the constitution of glucosides. Next the path of absorbed sugar is traced to the liver, and his experiments proving its conversion into glycogen are given. Many experiments upon the amounts of sugar present in arterial and venous blood, in blood from the portal and hepatic veins, and in the liver and other tissues, are given, as well as quantitative results of the amount of glycogen and other carbohydrates present in these positions. The sugar in urine is next discussed, and here he brings evidence to prove that normal urine contains sugar.

Dr. Pavy for his quantitative results relies almost entirely upon the ammoniated cupric solution, and contends that the results obtained by its careful use are thoroughly trustworthy. Where he gives duplicate estimations, however, we note that there are often considerable differences in the two values obtained. Thus, his figures give a further confirmation of the difficulty, if not impossibility, of obtaining very accurate results as to the quantity of sugar present in a solution, by means of a standard copper solution. It is of essential importance to keep this constantly in mind in those cases, such as determination of the quantity of sugar in blood from portal and hepatic veins, or from carotid artery and jugular vein, in which the results, which we know must be very close to one another, are to be compared. For this reason especially we do not consider that Dr. Pavy has satisfactorily established his position, though it is a most important one for his further conclusions.

His experiments upon the glucoside constitution of proteids are very suggestive and important, but before they could be conclusive it would be necessary to pay much greater attention to the proper purification both of the proteid with which he starts and the "cleavage carbohydrate" which he obtains, and to give quantitative analyses of this latter body. For instance, in the case of egg albumin, which is one of the bodies he uses, we know that it contains a considerable amount of glycogen, and that it is extremely difficult to completely free the proteid from it. But as this latter point does not seem to have been thoroughly attended to in these experiments, we must await further corroboration.

There are many other points about which we expect much discussion may arise, and among these we may mention his conclusions—firstly, that sugar is present in urine; and secondly, that it is present in proportion to the amount present in the blood. In connection with the first of these two points we would refer to the recently published paper of Carl Baisch,[1] who uses the benzoyl chloride method, which is certainly to be regarded as capable of producing more accurate results than the use of the copper test. He, in confirmation of Dr. Pavy, finds that a reducing carbohydrate is present, and he further shows that dextrose is present to the extent of ·04 to ·09 per 1000 of urine, total-reducing carbohydrates amounting to as much as ·06 to ·16 per 1000. Dr. Pavy, on the other hand, finds a much larger quantity, which, calculated as glucose, he states, amounts to as much as ·5 per 1000.

Now, if we compare the figures given on page 189 as to the amount of sugar present in urine as a consequence of an abnormal small excess in the blood, with the result he gives as present in normal urine, we cannot, from their study, at all agree that these amounts are proportional to that present in the blood. Still less would this conclusion be acceptable were we to take the above-quoted figures of Baisch.

Perhaps the conclusions upon which Dr. Pavy would lay by far the greatest amount of stress is that the major part of the sugar absorbed from the alimentary canal is immediately combined with the peptone simultaneously absorbed to form a proteid, which is then transmitted to the general circulation, and thus to the tissues generally. Dr. Pavy considers that this synthesis is in part effected by the epithelial cells covering the villi. Under one set of conditions, viz. defective oxidation, &c., this proteid may break down and result in the production of fat, or again under another set, *e.g.* excessive oxygenation of the blood associated with vaso-dilatation, a second form of katabolism is produced, characterised by the production of sugar, which is then eliminated in the urine. Before, however, we can accept these views it will be necessary that much of Dr. Pavy's work should be corroborated. It is of course known, that under the right conditions within the body, fat or sugar can be formed as a result of unusual proteid katabolism; but that proteid is usually syn-

[1] *Zeit. für Physiol. Chemie*, xix. p. 357.

thesised in the simple manner suggested by Dr. Pavy, namely, by the direct combination of a glucose molecule or molecules with a peptone or some similar molecule, requires far more rigid proof than is here attempted.

We should have liked to have seen a greater amount of criticism of some of the many important researches which have been lately published by so many workers; but throughout the whole work, Dr. Pavy bestows but little space in criticising conflicting evidence, much of which remains completely unnoticed.

Though it is thus easy to criticise and point out where errors may have crept in, we can but thoroughly admire the energy and careful thought which are in evidence throughout the work. There is much in it that is suggestive, much that is most valuable. The experimental work which is here set forth so plentifully, forms a field which must be studied and consulted by all future workers in this direction.

PTOLEMY AS A PHILOSOPHER AND ASTROLOGER.

Studien über Claudius Ptolemäus; ein Beitrag zur Geschichte der griechischen Philosophie und Astrologie. Von Franz Boll, Dr.Phil. (Leipzig: Druck und Verlag von B. G. Teubner, 1894.)

IT is somewhat strange that in the article on Ptolemy in the "Penny Cyclopædia," he is spoken of only as a geographer. His fame is undoubtedly built upon his two great works on astronomy and geography. But the present publication treats of him rather as a philosopher, and discusses also the genuineness or otherwise of the less-known works of the great Alexandrian. A few lines are devoted to his life, of which scarcely anything is known. Dr. Boll sees no reason for calling in question the statement of Theodorus Meliteniota, that he was born at Ptolemais Hermii, in Upper Egypt. He lived to his seventy-eighth year, and died in the reign of Marcus Aurelius, who became emperor in A.D. 161; but it is somewhat doubtful whether the last observation referred to in the "Almagest" was made in 141, or ten years later. At any rate, it is clear that that work preceded the description of the earth's surface (written, Sir E. Bunbury remarks, much more in the spirit of an astronomer than of a geographer), which remained during more than twelve centuries the paramount authority in geographical questions where physical matters were not concerned.

However, as we have said, the present treatise is not occupied with any consideration of these great works. Astronomical and geographical questions do not form its subject-matter, which is rather concerned with the comparatively untrodden ground of Ptolemy as a philosopher, besides a discussion of the genuineness of his writings on astrology in the present acceptation of the term. His work, Περὶ κριτηρίου καὶ ἡγεμονικοῦ, has been generally overlooked, the first apparently to refer to it being Heinze, in his edition of Ueberweg's "Grundriss der Geschichte der Philosophie von Thales bis auf die Gegenwart." But besides this professed treatise, Ptolemy touches upon philosophical questions in several places in his other writings.

Dr. Boll's "Studien" is distributed into three principal sections. The first gives, in three chapters, a discussion of his author's views on questions of this nature, as they may be derived from passages in his undoubtedly genuine works. The second is devoted to a critical examination of the genuineness of the Τετράβιβλος Σύνταξις, which is astrological, and generally considered as unworthy of the writer. An inquiry into the source of the so-called astrological geography in the second book of that treatise forms the third section of the present "Studien" of Dr. Boll.

Our space in this brief notice enables us only to indicate the general conclusions arrived at under these three heads. A detailed discussion of the views propounded by Ptolemy on psychological and other philosophical questions shows that he must be classed as an eclectic, but with distinctly peripatetic principles. This appears in the "Harmonics" as well as in the work first mentioned. But in his teaching a number of stoical propositions is also to be found, and it is not in these only that Aristotelian ideas are rejected or set aside, for a tendency is manifested to accept some Platonic doctrines on psychology, whilst Pythagorean speculations on numbers form the foundation of the third book of the "Harmonics."

A very careful and elaborate comparison of expressions used in the Τετράβιβλος, and in the smaller work Καρπός (which is in fact a collection of aphorisms), with Ptolemy's great astronomical and geographical works, proves that the former is genuine, but the latter is not, being evidently the production of some astrologer of later date.

Dr. Boll's conclusion with regard to the Τετράβιβλος agrees with that of previous writers; thus the author of the article "Astrology" in the "Penny Cyclopædia" says: "Though its genuineness has been doubted by some merely because it is astrological, there appears no sufficient reason to reject it." We would gladly do so if we could; but the present examination seems to confirm the Ptolemean authorship but too fully. The admiring English translator in the last century (John Whalley, Professor of Physic and Astrology) affirms that "there is nothing in Astrology but what is there comprehended, nothing there comprehended, but the Quintessence and Divinity of Astrology."

The second book of this treatise gives a system of what Dr. Boll calls astrological ethnography, *i.e.* the stellar influence on different parts of the world and their inhabitants according to the signs of the zodiac which are especially supposed to rule over each. The sources of this, by a comparison with earlier writers, are discussed by Dr. Boll (led so to do by a small treatise published at Berlin by Paul Wendland in 1892) in the third section of his work, which shows great industry and research, it being difficult to disentangle the subject from that of the effects of climate on the human race, so that matters of this kind require very careful handling. We may take an instance of this from our own Shakespeare. When Prospero says his zenith doth depend upon a most auspicious star, the allusion to astrology is patent. But when the melancholy Jaques, in *As You Like It*, is made to describe human life as divided into seven stages, it is somewhat straining a point (Dr. Boll refers to Steevens as having anticipated

him in this, but he means Malone) to assume that reference is intended to the notion that each age was dominated by one of the seven planets. However, he shows a close correspondence between many expressions in the Τετράβιβλος and in Roman writers under the empire, Manilius, Vitruvius, and Pliny.

The work concludes with an *excursus* on the date of the ἀστρολογούμενα of Petosiris and Nechepso, which had been referred to the first century before, but Dr. Boll gives reasons for placing in the first century after, the Christian era. W. T. L.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Platinum Resistance-Thermometers.

AT the meeting of the British Association, just coming to an end, it was pointed out to the Committee of Section A, by Mr. E. H. Griffiths, that the general adoption of the method of thermometry, founded on the variation of the electric resistance of platinum with temperature, that has been worked out by Prof. Callendar and himself, is seriously hindered by the existence of a report presented to the Belfast meeting of the Association in 1874 (British Association Report, 1874, pp. 242-249), by a Committee "appointed for the purpose of testing the new pyrometer of Mr. Siemens." As I was secretary of this Committee and drafted the report, and as all the experiments were made either by myself or under my direction, I was desired by the Committee of Section A to ask you to allow me to state in the columns of NATURE (what is indeed obvious to anyone who refers to the 1874 report) that the tests carried out by the Committee of 1874, and the conclusions arrived at by them, had reference solely to the pyrometers supplied to them for examination by Messrs. Siemens Brothers, and that they have consequently no bearing on the question of the trustworthiness or accuracy of the platinum resistance-thermometers of the kind devised by Messrs. Griffiths and Callendar.

August 15. G. CAREY FOSTER.

International Courtesy.

I LEARN from a speech of Prof. Ludwig Boltzmann, in Section A to-day, and also from some Englishmen well acquainted with German Universities, that I have unintentionally offended the physical philosophers of Germany by one or perhaps two ill-considered and hasty expressions employed in the first edition of my "Modern Views of Electricity."

These remarks do not occur in the second edition, but mere silent withdrawal of them does not convey the information that I desire to convey to my illustrious leaders and *confrères* in the foreign scientific world. I therefore request you to permit me space to make the following *amende*.

When I said that the four great names in connection with our partial knowledge of the nature of electricity were (excluding living persons) Franklin, Cavendish, Faraday, and Maxwell, I ought to have interpolated the adjective *British* before the word "names," in order to avoid entering upon much larger questions than were at all appropriate to the expository course of lectures on which the book "Modern Views" was based.

The second remark was this:—When emphasising the great achievements of Hertz, in my lecture at the Royal Institution on "The Discharge of a Leyden Jar" (reprinted as appendix to same book), I spoke of him as "no ordinary German." Literally of course it is true, but it may easily be interpreted in a discourteous sense. It was, however, less widely known then than it is now that Hertz was a German savant of the highest type, and this fact I wanted to express; but if the proverbial odiousness of implied comparisons had only happened to strike me, I would certainly have altered the mode of expression before any reprints of my lecture were made.

Prof. Boltzmann seems to think that the context to this remark indicates that some rancour was felt in this country that the fruits of Maxwell's theory should have been reaped by a German. That, if true, would be a serious accusation, but I can assure him that it is conspicuously untrue. To an Englishman my words would not even convey the impression. I honestly think that at the present era no trace of international jealousy exists among English and Irish physicists.

August 14. OLIVER J. LODGE.

A Remarkable Meteor.

WHILE at Pasadena, eight miles north-east of Los Angeles, California, on July 27, a few minutes before half-past seven, the writer had his attention suddenly drawn towards the north-western horizon by a bright flash of light as of the bursting of a meteor; but on keeping his gaze fixed on the point where this flash appeared, he was surprised still more to see that instead of disappearing, as usually happens with meteors when they explode, there remained a very luminous figure, somewhat of the shape of the new moon but with more wavy outlines, and of an intense whiteness, something as of an electric light, in well-defined relief against the pale golden glow of the sky. The whole time during which this luminosity was visible was something over twenty minutes, and it had ceased to be visible at eight minutes before eight. The crescent shape was not maintained more than about three minutes, then it took the appearance of a luminous vapour or cloud rising vertically for a little distance and then bending off sharply to the left in almost an horizontal line, but not showing any tendency to dissipate or grow thinner at the end farthest from the point of origin. As time went on, the whole figure became more wavy in outline, but persistently remained fixed in the same part of the sky. The bottom, the point of origin, was slightly brighter than the rest of the figure, and a little reddish in colour, and the underside of the arm outstretched to the left was brighter than the upper side. It was clearly beyond, and in no wise connected with, the Sierra San Gabriel, which cut the sky with a dark, well-defined lune under the luminous figure. All who were watching it perceived that it was no common cloud; the north-western sky was cloudless and free from haze, and no cloud in the west at such an hour can shine with this sort of light, which indeed had more the lustre of white flame.

The cloud seemed unbroken so long as it was visible at all. It would be idle to speculate beforehand on the exact locality of this outburst, since no accurate estimate of its distance could be formed at the time; but the direction, as nearly as the writer could judge by reference to the pole star, was about north 35° to 40° west, which, projected on a map of the State, gives about the direction of Tehachipi Peak from Pasadena.

The direction in which the meteor was seen to explode, as stated by other observers all the way from the Needles in the south-eastern part of the State to Lodi and Oakland in the central counties, that is to say, from points five hundred miles apart, enables one, by projecting those bearings which are reported in most detail, viz., Fresno, whence it seemed a little north of west; Keeler, whence it also seemed a little north of west and directly beyond Mount Whitney; Tracy, whence it seemed to be in the south-east; and Pasadena, whence it seemed, as above stated, to limit the spot over which it exploded to some point in the north-western part of Fresno county or the south-western part of Merced county, both being in the San Joaquin Valley, and 250 to 300 miles from Pasadena. With much greater diffidence the writer would estimate its angular height above the true horizon at not to exceed 3°.

Los Angeles, July 29. EDWARD WESSON.

[From newspaper cuttings sent with the foregoing it appears that the meteor was seen at the Lick Observatory at 7h. 35m., and that the explosion was heard at 7h. 36½m. At Benicia the meteor was extremely brilliant for a moment, and then disappeared in a column of white vapour about two degrees long. This cloud remained visible for a quarter of an hour. At Fresno and at Redlands the luminous stream was visible for twenty minutes. An observer at Tracy says that a loud report, resembling a clap of thunder, was heard in the south-east,

five minutes after the meteor disappeared. According to the news from Fresno, the meteor left a track of great beauty, consisting of an irregular spiral curve, the lower end of which was little more than a tangle of threads. The upper part of this track was pale red, and farther down blue. The lower part was almost yellow, and still farther down were two detached bright red spots, like the sun breaking through clouds.—ED. NATURE.]

Height of Barometer.

MR. PEARSON will find much information as to extreme readings of the barometer in two papers published in the *Quarterly Journal* of the Royal Meteorological Society; one by Mr. H. Sowerby Wallis, in vol. viii. p. 147, and the other by Mr. C. Harding, in vol. xiii. p. 201. The lowest known reading is stated, on the authority of Mr. Blanford (NATURE, vol. xxxv. p. 344), to be 27·135 ins. observed on September 22, 1885, at False Point, on the coast of Orissa; this requires a subtractive correction of ·011 to bring it to English standards, reducing it to 27·124 ins. The highest known reading is given, on the authority of Prof. Loomis, as 31·72 ins. at Semipatalinsk, on December 16, 1877, giving an extreme range of 4·6 ins.

The lowest reading recorded in these Islands is 27·332 ins. at Ochtertyre, near Crieff, on January 26, 1884, while at Belfast the barometer fell to 27·38 ins. on December 8, 1886, and on the same day at Newton Reigny to 27·566 ins., which seems to be the lowest recorded in England. The highest pressures recorded in this country during recent years were on January 18, 1882, when 30·990 ins. was registered at St. Leonards, but on January 9, 1820, 31·056 was recorded at Kinfauns, Perth, and appears to be confirmed by other readings in Scotland.

HENRY MELLISH.

THE BRITISH ASSOCIATION.

BY the kindness of the Secretary of the British Association we were able to give in our last issue a list of the grants awarded by the General Committee just as we were going to press. Upon referring to this, it will be seen that the grants amount to very nearly £1100, that is £400 more than those awarded at the previous meeting. The increase of funds available for research is due to the large number who attended the Oxford meeting, the receipts being as much as £2175. In this matter, and indeed from every point of view, the meeting was a most successful one. The membership reached a total of 2321—a number greatly in excess of the average. In moving a vote of thanks to the authorities of the city for the hospitable reception accorded to the Association, Sir John Evans remarked that the meeting had been notable both for the large attendance of members and associates, and for the great scientific interest and importance of the papers read. In fact, it was the opinion of all that rarely, if ever, has a more brilliant meeting of the Association been held. No less than seventy-seven foreign members, eminent in many branches of scientific knowledge, honoured it with their presence. The exchange of ideas, which results from the meeting of investigators from all parts of the world, must lead to real progress. "Science," as someone has said, "is cosmopolitan." She recognises no differences of nationality between workers devoted to extending her domains. Therefore men who live "for the promotion of natural knowledge" meet on common ground at the British Association, for they know that anything that will help on this object will be appreciated.

Several changes in the constitution of the sections were adopted by the General Committee. Section D will in future be called Zoology instead of Biology, and there will be a separate section for Botany. Section I, which met this year for the first time, is to consist of Physiology, with Experimental Pathology and Experimental Psychology. As pointed out by Prof. Bayley Balfour in his address, Section D has had its constitution changed oftener than any other section of the Association. Experience will show whether the new arrangement is the one best calculated to bring together investigators with similar scientific interests.

The continual division of this section suggests that Astronomy should be removed from Section A (Mathematical and Physical Science), and have a section of its own. It may also be well to point out that there should be a sub-section of Section H (Anthropology) dealing with large questions of Archæology—that is to say, with Assyrian and Egyptian Archæology—and with the various points which, from an archæological point of view, are common to the earlier races.

Another matter worth the attention of the General Committee is the introduction of evening reunions of physicists and biologists, such as are provided in German meetings. Under the present conditions it is very difficult to meet and talk with fellow-workers, especially with foreign members, at each meeting.

The meeting will be held next year at Ipswich, under the presidency of Sir Douglas Galton, K.C.B., F.R.S. Liverpool will be the place of meeting in 1896. The Association was invited to meet in Toronto in 1897, but as arrangements are never made more than two years in advance, nothing definitely could be settled in the matter. There was a strong feeling, however, in favour of accepting the invitation when the proper time arrives for doing so.

The University testified to its interest in the welfare of science by conferring the degree of D.C.L. *honoris causâ* on the following eminent foreign investigators present at the meeting:—Prof. Edouard Van Beneden, Prof. Ludwig Boltzmann, Dr. E. Chauveau, Prof. Cornu, Prof. Theodor W. Engelmann, Prof. Wilhelm Förster, Prof. C. Friedel, Prof. L. Hermann, Prof. Gosta Mittag-Leffler, Prof. S. P. Langley, Prof. G. Quincke, Prof. E. Strasburger. The degrees were conferred by the Vice-Chancellor, and the Latin oration was delivered by Prof. Goudy. The following brief notes show the character of the recipients' chief researches:—

Edouard Van Beneden, Professor of Zoology and Comparative Anatomy, has not only contributed a long series of memoirs on the structure of various Invertebrata to the literature of zoological science, but has especially gained the highest recognition and esteem for his work on the microscopic details of the process of fertilisation in relation to karyokinesis and cell-structure. His investigations on this process in Ascaris megalocephala form the starting-point of recent theories and researches on the subject of the partition of the nuclear matter by the splitting of the chromosomes in spermatozoon and ovum and in the fertilised egg. In addition to these investigations, Prof. Van Beneden's researches on the formation of the blastoderm in the rabbit and the bat have been of the greatest importance, and are cited in all modern treatises as classical. Recently, Prof. Van Beneden has occupied himself largely with the study of Anthozoa (especially Cerianthus and its larva Arachnactis), and has arrived at most important conclusions as to the relationship of these forms to the Vertebrata.

Ludwig Boltzmann was born in Vienna in 1844, and is now Professor of Theoretical Physics in the University there. His first paper was on the distribution of electricity on a sphere and cylinder, and his second one on the mechanical significance of the second law of Thermodynamics. His subsequent papers are too numerous to mention in detail, and have been published principally by the Academy of Science at Vienna, and recently at Munich. The most important of these treat of the steady state of kinetic energy in gas molecules and its connection with the second law of Thermodynamics, of the specific inductive capacity of solids and gases, and other thermodynamic and electromagnetic subjects. Along with Clausius and Maxwell, he is a founder of the kinetic theory of gases, especially in its more com-

plicated aspects and in its connections with the second law of Thermodynamics. Recently he has devoted himself to popularising Maxwell's electromagnetic theory in Germany.

French physiologists are represented by Prof. Chauveau of Paris. Two only of his achievements need be mentioned —his investigations of the movement of the heart, conceived in the same spirit, and pursued with the same desire to search out the secrets of nature that animated our own Harvey; and secondly, his inquiries as to the nature of the process by which infectious diseases are communicated from one individual to another. It would be difficult to estimate which of these is most worthy of our admiration, for whereas the first were experiments of light, the others were experiments of fruit, and served, with those of Pasteur and our own Lister, as the foundations of a new science—that of Bacteriology —pregnant with promise for the future welfare of mankind.

M. Cornu, Professor of Physics at the École Polytechnique, Paris, is renowned for his numerous experimental researches. His investigations on the velocity of light earned for him a high place among experimenters twenty years ago, and his work on the ultra-violet part of the solar spectrum is still the standard of reference. The telluric spectrum also, and the spectrum of hydrogen, have engaged his attention, as well as various problems in astronomical physics. But his investigations have not been confined to optical physics, one of the most important of them being concerned with the determination of the density of the earth. He was elected a Foreign Member of the Royal Society in 1884, and received the Rumford medal in 1878.

The career of Prof. Engelmann of Utrecht as an investigator has been very fruitful. His work, like that of Hermann, has related to the very *principia* of physiology—to those vital endowments which are common to ourselves and to organisms of the simplest structure. These he has studied with a view to the eventual solution of the most elementary, yet the most difficult, problems which living nature presents to the investigator.

Prof. W. Förster, the Director of the Observatory of Berlin University, is well known for his great activity in furthering astronomical inquiry, both in the institution under his charge and elsewhere. Quite recently he succeeded in establishing an International Bureau for undertaking and conducting astronomical computations. He has also played an important part in the work of the Geodetic Union. Most of his earliest work belongs to geodesy, a number of carefully-made pendulum observations calling for special mention. By directing attention to luminous clouds, and pointing out the importance of photographing and accurately observing them, he has done a service which will lead to results interesting alike to astronomers and meteorologists. His astronomical works, though not numerous, are such as add to his renown, and, with his rare and active administrative faculty, they single him out as well deserving the honour done to him.

Prof. Friedel, the eminent occupier of the chair of Organic Chemistry at the Paris Sorbonne, and one of the six members of the Chemistry Section of the Paris Academy of Sciences, has carried out numerous investigations of the highest value. His first work was on the relations between thermo-electric properties and crystalline form, but his chief researches relate to organic compounds, in the synthesis of which he has been very successful. A few of his earlier papers refer to the artificial production of minerals, and he has made some important contributions to the inorganic side of chemistry.

The name of Prof. Hermann of Königsberg is familiar to students of physiology in all parts of the world, as the author of a general treatise on the subject which has been translated into every European language. He is also the author of several monographs on special subjects, and of innumerable smaller papers, in each of which some permanent additions to knowledge are recorded. His investigation of the chemical processes which are concerned in muscular contraction, published a quarter of a century ago, and his more recent inquiries as to the electrical concomitants of these processes, constitute the foundation of what Physiology is as yet able to teach on these difficult but fundamental questions.

It was at the Allegheny Observatory, Pennsylvania, that Prof. S. P. Langley began his investigations in solar physics, which have resulted in so great an extension of our knowledge in this direction. About 1878 he turned his attention to the question of solar radiation. Finding the thermopile quite inadequate for the work he had undertaken, he was led to invent his now well-known bolometer, with which instrument he has since carried out some very important investigations. After the death of Prof. Henry, he was offered the position of Assistant Secretary of the Smithsonian Institution by Prof. Baird. He removed to Washington, but for a time continued to carry on his work at Allegheny. A few years ago, however, he built a small astro-physical observatory on the grounds of the Smithsonian Institution. and he has there continued and extended his work on the infra-red spectrum of the sun.

All mathematicians are familiar with the name of Gosta Mittag-Leffler, Professor of Pure Mathematics in Stockholm University. He is the editor of the authoritative journal *Acta Mathematica*, devoted to the extension of mathematical knowledge. His work in pure mathematics has been of a very varied character, the most noteworthy, perhaps, being concerned with linear differential equations and their integration, and with the theory of the uniform functions of a variable.

The Royal Society elected Prof. Georg Quincke of Heidelberg as a Foreign Member in 1879, for his researches in physics. He is one of the veterans of science, his first paper having been published so long ago as 1856. All branches of physics have been benefited by his careful experimentation and acute reasoning. Few workers, indeed, can claim to have added so much as he to our knowledge of physical laws, or to have studied natural phenomena in a more comprehensive and profound manner. His researches on capillarity, carried on thirty-seven years ago, led up to the important work in which he showed that the movements of amœbæ and protoplasm can be fully explained by physical laws.

Prof. Strasburger is Professor of Botany in the University of Bonn, having previously been Professor in the University of Jena. He is a Foreign Member of the Royal Society. Having made his mark in Morphology by his monumental work on the Gymnosperms ("Die Coniferen und Gnetaceen," 1872), he has since chiefly devoted himself to the study and investigation of the nucleus of the cell, with special reference to the reproductive processes of plants in their connection with the phenomena of heredity. His researches fill several volumes, such as "Zellbildung und Zelltheilung," "Angiospermen und Gymnospermen," "Unters. ueb. d. Befruchtungsvorgang," and his recent "Histologische Beiträge." It is not too much to say that to Prof. Strasburger's researches is due nearly the whole of our present knowledge of the processes of cell-division in plants.

SECTION I.

PHYSIOLOGY.

OPENING ADDRESS BY PROF. E. A. SCHÄFER, F.R.S., PRESIDENT OF THE SECTION.

BEFORE beginning the subject matter of my address I had conceived it to be necessary, appearing before you as we do as a new Section, to offer some sort of apology for our presence. But, on looking up the history of the Association, I find that my task is somewhat different. If I have any apology to offer

at all it is that the Section of Physiology has ceased to appear for many years.

The British Association was founded at York in 1831; and at the subsequent meeting, which was held in this very city of Oxford, amongst other Sections which were established, was one for Anatomy and Physiology. Now, when we consult the records of this Section we are struck with the fact that Medicine early shows a marked preponderance. Thus, in 1833 a physician is selected as President for the Section, with two surgeons as secretaries; one of them, be it noted, being Mr. Paget. This preponderance soon came to be recognised in the designation of the Section, for in 1835 we find it entitled Section E, Anatomy and Medicine.

As time went on the interests of medical men became gradually more absorbed in the rapidly growing British Medical Association; and in 1841 the medical title was dropped, and the Section came to be called simply Physiology, which title it retained until 1847. Under that designation the Section has now been revived.

The fact that Physiology as a separate section in this Association was allowed to lapse for so long a period is not remarkable when we remember that during the first half of this period Physiology as a science was practically non-existent in this country. The teachers of Physiology were, almost without exception, practising physicians and surgeons, and even when a professor was expected to devote the whole of his time to the teaching of Physiology he was not expected to devote part of that time to the prosecution of physiological research. During all these years, from 1833 to 1847, we do not find amongst the officers of the Section any actual working physiologists. Most of the officers were distinguished medical men, with an anatomist here and there amongst them. Far be it from me to say that there was no actual work being done in Physiology at this time; for Charles Bell and Marshall Hall were engaged in elucidating the functions of the nervous system; whilst Bowman, Wharton Jones, and others were producing good and permanent work in various other departments of Physiology. Their labours, however, were isolated, and formed but oases in the Sahara of neglect into which the pursuit of Physiology had fallen in this country; and this during a period when it was being pursued with signal success and activity both in Germany and France.

After 1847 a revival of Physiology began to manifest itself even here; and this was followed by the establishment, from time to time, of a sub-section to Second D, which was devoted to Physiology, and had a special President. Whether, however, owing to their subordinate character, or from some other reason, these sub-sections had not usually any great measure of success, and for the last twelve years they have been wholly dropped. During that period Physiology has only twice been represented in the chair of Section D, and has usually had no secretarial representation. This decadence of Physiology in the British Association during the last eleven or twelve years is the more remarkable because it is obviously not due to any want of outside activity in regard to the subject; for during this period we find an extraordinary revival of interest in physiological research, a revival which in its most active stage dates from about twenty-five years ago, but still some twenty or thirty years later than the corresponding revival in France and Germany. I have taken the trouble to prepare a list of prominent physiological workers who flourished during the thirty years prior to 1870. My list comprises, in all, thirty. Of these four are English, five French, and twenty-one German or Dutch. Of the four English working physiologists not one is a teacher of Physiology. Of the five French and twenty-one German all are recognised teachers. It was not, in fact, until it came to be understood that teaching and work in Physiology, as in all branches of science, ought in the main, to be successful, to go hand in hand, that the science had any possibility of revival.

Let us glance for a moment at the history of the revival of Physiology in this country as compared with its revival in Germany. In each country the revival may be said to have been largely due to the influence of one teacher. In Germany the teacher was Johannes Müller; in this country, William Sharpey. Both of these remarkable men were pupils of Rudolphi, who was professor of Anatomy and Physiology in Berlin until 1833. It is stated regarding Rudolphi that "he was an enemy to subjective speculation in biological science: he looked on the so-called philosophy as mistaken and futile in its application to the phenomena of the animal economy, and based his physiology chiefly, and perhaps rather exclusively, on the study of the animal structure." The influence of Rudolphi is apparent in both Müller and Sharpey.

Müller was born in 1801, Sharpey in 1802; they were therefore of about the same age. But Müller's scientific and intellectual development was more rapid than that of his contemporary. Thus we find that already in 1826, when he was but twenty-five years old, Müller attained so great a reputation as to be made Professor Extraordinary in the University of Bonn; and before very long he was promoted to the grade of Ordinary Professor there. In 1833, whilst still a young man, he was called to the chair of Anatomy and Physiology at Berlin, which had just become vacant by the death of his master and friend, Rudolphi. Sharpey, on the other hand, occupied himself until 1829 with perfecting both his general and his special anatomical education. It was not until 1830 that he published his first essay in anatomical and physiological research entitled "On a Peculiar Motion excited in Fluids by the Surfaces of Certain Animals"—observations which were preliminary to the discovery of the existence of cilia in vertebrates. And it was not until 1836 that he was called to the newly instituted professorship of Anatomy and Physiology in University College, London, which he filled for so many years with such signal success. Both of these distinguished men owed, there is no doubt, their success as teachers of Physiology to their early anatomical training. The general anatomical bent of Johannes Müller is evidenced by the fact of his scientific work being turned so much in the direction of comparative Anatomy and Physiology. And Sharpey, although great, and deservedly great, as a teacher of Physiology, remained to his dying day, above all, an anatomist. Physiologists of this school are rare at the present day; but it is probable that in some respects the progress of Physiology may suffer thereby. Helmholtz began his public career as professor of Anatomy; but it would be unfair to attach too much weight to this particular incident in the case of so many-sided a man as the great Berlin Professor of Physics. Nevertheless, the necessity of a close and careful training in Anatomy for those who are afterwards to work at or to teach Physiology is so important that I do not hesitate to say that the younger physiologists who neglect the study of Anatomy will find that before very long they must abandon the pursuit of many byways of Physiology which might otherwise be followed up with manifest advantage.

The influence of Johannes Müller upon the revival of the pursuit of scientific Physiology in Germany, and indeed generally, cannot be over-estimated. We have only to look at the names and eminence of his pupils in order to recognise the immense influence which his teaching has exerted upon the progress of Physiology ever since his time. Some of these pupils are still amongst us, others have joined the majority. But the pupils of these men, again, are now great names in many departments of our science, and through them we cannot fail to recognise the influence which was exerted by this truly great man.

We may say the same in almost identical words of William Sharpey. The practical pursuit of Physiology in this country has mainly radiated from the centre where Sharpey taught. Michael Foster was his pupil. The physiological investigations of Burdon-Sanderson were assisted and encouraged by him. From Sharpey, therefore, we may trace the rise of the great school of Physiology at Cambridge, and we have only to look at the magnificent laboratory which has been erected here to observe a monument of the influence of the same teacher. And there have emanated either directly from the physiological school established by Sharpey at University College, or indirectly from those at Cambridge and Oxford, many of the most active teachers and workers in Physiology in the kingdom.

In these respects there is much in common between the revival of Physiology in Germany and in this country. In other respects, however, the two cases have been entirely under different conditions. There its revival, in common with that of science generally, has been assisted and stimulated by the active and beneficent co-operation of every German State. Here, also, in common with science generally, it has had to make its way against every conceivable obstacle; and almost without assistance, either moral or material, from the Government or from public bodies. But not only has it not met with assistance, there have been actual obstacles placed in the way of teaching and work in Physiology. Some have been unintentional, others

intentional. As an instance of the unintentional may be mentioned the practice which has obtained in medical schools and on examining boards—a practice which, I am happy to say, is gradually being discarded—of appointing as teachers and examiners in Physiology men who may have a good general knowledge of the science, yet with whom it is not the business of their lives; and who cannot, therefore, be expected to be as familiar with its details, and absorbed in its interests, as those who devote their entire time and attention to its pursuit.

The more virulent opposition, in some measure, to science generally, but in the greatest measure to Physiology, appeared almost simultaneously with the active revival of the subject. This opposition, which has come to be known as the Anti-vivisection Movement, but which might equally well be called the Anti-scientific Agitation, has hitherto met with no measure of success, except that it has to a certain extent hampered the full development of the science by diverting to its defence some of the energy which might be devoted to its pursuit. Indeed, the actual results of this unreasoning agitation furnish an illustration of the old-established principle that persecution of a good cause will in the long run tend towards its development and propagation. And in this case the chief results have been the following:—

(1) The most immediate effect of the anti-vivisectionist attack was the establishment of the Physiological Society, which in the first instance was only a small gathering of working physiologists, who met to discuss measures of defence in a drawing-room in Queen Anne Street. This society, which had such a small beginning, is now large and important. Its doors are besieged by applicants for admission, although it is a necessity for such admission that the applicant be either a teacher of Physiology or a worker at Physiology, or both. Its numerical strength has grown from ten to fifteen to more than 150; and its numbers are every year increasing. And, besides the work which it has done in this country in promoting the interests of Physiology, and co-operation between English physiologists, it has succeeded in establishing a succession of triennial International Congresses of Physiology, which are amongst the most successful of such gatherings, and which have been the means of bringing us into communication with the most prominent physiological workers and teachers on the continent.

(2) A second result of the agitation was the passing of the so-called Cruelty to Animals Act. This Act, which was intended to restrict the performance of experiments upon animals, was in no sense called for, since it had been found by a Royal Commission that there was no evidence to show that there had been unnecessary experimentation upon animals, or any desire on the part of physiologists to neglect the use of anæsthetics. On the other hand, it is of inestimable advantage in that it gives the public a definite guarantee that the excesses of which physiologists used to be freely accused are not possible. Such excesses never did actually occur; although, to believe all the publications which have been issued by Anti-vivisection Societies, one would come to the conclusion that a physiologist is a being who spends his whole time in torturing sensitive creatures, careless of the suffering which he may cause, or even of the scientific results which he may obtain. The fanatical supporters of the agitation would have you to believe that we are all neither more nor less than "fiends"; they cry with Ferdinand that "hell is empty and all the devils are here."

I am told there was even a feeling of this sort in this University at the time when it was proposed to establish the Wayneflete Professorship of Physiology, and that an agitation was set on foot having for its object, first, the prevention of the establishment of such professorship; and secondly, that being impossible, the prevention of the professor's practising physiology. The common-sense of the University stifled this agitation, and the more intimate acquaintance with physiologists, which has resulted from the establishment of the school, has been sufficient, I believe, to smother the little fire which was still left smouldering.

(3) A third result of the Anti-vivisection Agitation was the establishment of the Association for the Advancement of Medicine by Research. This immediately followed a unanimous resolution of the International Medical Congress of 1881, affirming the necessity of experiments upon animals. To the ignorant accusation that physiological experiments had been and were of no use or influence in the advancement of medicine, the leaders of the profession unanimously affirmed that it is upon Physiology hat Medicine and Surgery are based, and that there can be no real progress in those sciences without a corresponding progress in experimental Physiology and Pathology. The Association for the Advancement of Medicine by Research has been of the greatest possible value and assistance to Physiology in this country. It has shown physiologists that they have the great medical profession at their back, and it has acted as an impartial and independent medium of communication between physiologists and the successive Secretaries of State, whose business it has been to administer the Act.

(4) A fourth result of the attacks of the anti-vivisectionists has been, I may perhaps be permitted to believe, the re-establishment of this Section of Physiology of the British Association. Those who were present at the meeting of the Association in Nottingham may have remarked that the gutters of that town were strewn with papers which had been forced upon the members of the Association by the anti-vivisectors of the place. This literature, which in a double sense may be termed "gutter literature," teemed with flagrant misstatements, and with vicious calumnies, directed against physiologists, and especially called forth, I presume, by the fact that for the first time in the history of the British Association a physiologist was called upon to occupy the presidential chair. We may look upon the establishment of this Section as the reply of the Association to the false witness which was borne against us at Nottingham.

But although a special section for Physiology has been re-established, it may not be advantageous that there should be one at every meeting of the Association. Physiology is above all things a practical science. It requires laboratories and means of demonstration. Physiologists are rarely satisfied with the opportunity of hearing and reading papers, but require that, as much as possible, the actual methods of research employed should be capable of demonstration. By this I am not to be supposed to advocate the demonstration of experiments upon animals, for there are very many subjects in Physiology which can be both worked at and illustrated in a manner involving in no sense whatever the word vivisection. But in order that the methods should be shown, it is important to have the appliances of a laboratory at hand, and the Association frequently meets in towns which are not university towns, and have no laboratories, in which, therefore, it would be difficult or impossible to arrange for demonstrations of the sort that I am alluding to. On this account we may well imitate the practice of the British Medical Association, which establishes a Section of Physiology only when its meetings are held in such a centre as is likely by the applicances which are to be found in that centre to render the Section useful and efficient. Hence, in recommending the establishment of a Physiological Section, it is expressly reserved that the Section shall be held only at such future meetings as may seem to the council to be desirable.

I will now invite you to consider with me one or two of the more obscure subjects in the range of Physiology, subjects which are, however creating a great, almost an absorbing, interest at the present moment The first of these subjects relates to the structure and function of every cell in the body. All are aware that the body of every animal and of every plant is made up of minute corpuscles which are formed of protoplasm, and which contain in every case at least one nucleus. The protoplasm and the nucleus form the living substance of the cell. Other substances may be present, but they are, in a sense, outside the nucleus and protoplasm, not incorporated with their substance. Apart from a few details relating to the structure of the nucleus, this was, until quite lately, practically all that we knew regarding the parts composing either the animal or the vegetable cell. There appears, however, to be yet another something which, although in point of size is of very insignificant dimensions, yet in point of function may perhaps be looked upon as transcending in importance, in some respects, both the protoplasm and the nucleus. Not many years ago it was noticed by various observers that in certain specialised animal cells the protoplasm showed a tendency to radiate from or converge towards a particular point, and on further investigation it was found that at this point there was a minute particle. This observation, which began, as we have seen, upon specialised cells, was, after a little while, found to hold good for other and yet other cells, until, at the present time, we believe that in every cell of the animal or plant body such a particle exists Now, it may well be asked, why after all should so great importance be attached to this observation? To this it may be replied that, in the first place, it is of importance, because it shows conclusively that the

whole cell is not of a uniform nature, since there is this one point within the cell that exerts a special attraction upon the rest of the cell-substance; and, indeed, on this account the particle has come to be termed the "attraction particle." And in the second place, because of the apparent universality of the occurrence of such a particle. And, thirdly, because of the fact that one of the most important phenomena exhibited by the cell hinges upon the behaviour of this particle; for it is found that before a cell or its nucleus divides this minute attraction particle begins by itself dividing, and is, in fact, more commonly met with double than single. Nor is it until the two particles thus produced have evolved, either from themselves or from the substance of the protoplasm or nucleus, a system of communicating fibres, the so-called *achromatic spindle*, that those changes in the nucleus and protoplasm take place which produce the division and multiplication of the cell. This *attraction particle*, which is also called the *central particle* or *centrosome*, has absorbed so great an interest that, short as is its history, many papers have already been devoted mainly to it, the latest of these being an elaborate treatise of some 300 pages by Martin Heidenhain. I shall not here attempt to follow out the details of all these researches, but will be satisfied with putting before you the conclusion which Heidenhain has come to regarding this particle, viz. "that it is morphologically, physiologically, and chemically a structure *sui generis;* not merely a separate portion of nucleus or of protoplasm, but an organ of the cell with definite functions, and having a definite existence of its own." Nevertheless, it is almost as minute an object as it is possible to conceive. In a cell which is magnified a thousand diameters the central particle appears merely the size of a pin-point. Yet this almost infinitely small object exerts an extraordinary influence over the whole cell, however large (and the cell may be many thousand times its size); for it initiates and directs those processes which result in the multiplication of the cell, and indirectly, therefore, it is concerned in directing the general growth of the individual, and ultimately the propagation of the species.

A former President of the Association took as the subject of his presidential address what he was pleased to call the "Next to Nothings." In considering this central particle, of the actual structure of which, and of its chemical constitution, we know at present hardly anything, we may surely regard it as a striking instance of the supreme importance of the "next to nothing" in Physiology.

The other subjects to which I desire to draw your special attention relate to the physiology of certain organs the functions of which have always been extremely obscure, and which, although they differ greatly from one another in almost every point of structure, and presumably also in function, it has been usual to group together under the name of ductless glands. The name "gland" is given to such organs of the body as take materials from the blood, and convey those materials in an altered or unaltered form, by a tube or duct, to a surface either internal or external. Such material is termed the secretion of the gland, and has for its object either the performing some function which is useful to the organism or the getting rid of material which would be detrimental if retained. In the case of the ductless glands there is no such possibility of pouring out material produced by the gland upon a surface, because these organs do not communicate with any surface by a duct; and whatever material they may furnish must therefore, if it is to reach the body generally, pass into the blood; that is to say, the blood on the one hand must furnish the materials for the secretion of the gland, and on the other hand it must take up those materials after they have been manufactured into something else, and carry them away to other parts of the body. Now, in the case of a certain number of the ductless glands there has not appeared to be any very great obscurity as to their function; for some of them seem very obviously to be devoted to the formation of corpuscles which are found within the blood itself. But with regard to others of these bodies it has not hitherto been possible to find any special material in the blood which they have furnished to it, and our knowledge of them is derived almost entirely from experiments. I will take the case of two of these to illustrate the vast influence which small and almost disregarded organs may exert upon the whole economy. But in the first place I may be permitted to point out what is indeed a self-evident statement, that there is no part of the body which does not exert some influence upon the rest. Every single portion of the body is continually taking materials from the blood, and furnishing to the blood other materials which are formed within it, whether we call that portion which performs such functions a gland or not; and it is quite certain that the removal of any portion of the body would be followed by some permanent alteration in the blood were it not that other similar parts may by increased activity compensate for the alterations which the blood would otherwise undergo from the loss of any one such part. Take the case of a limb. The changes which the blood undergoes in circulating through it affect the body generally through that fluid, for the composition of the blood becomes modified in traversing the limb. And not only is the body affected thus through the medium of the blood, but, by means of the nerves which pass to and from the limbs, the central nervous system is itself affected by the movements and alterations of various kinds which are proceeding in the muscles and other parts, and through the nervous system the whole organism must constantly be influenced from the limb. There is, however, no evidence that the removal of a limb or part of a limb permanently modifies either the condition of the blood or of the nervous system. Nor is such a result to be expected, for in this case there are other parts of the body possessing similar organs and performing similar functions, the increased activity of which may easily compensate for the loss which is sustained through removal of such a part.

But if we deal with an organ which is not multiple, but unique, and completely remove this from the body, it is easy to see that the case may be very different. This organ, like every other organ of the body, is continually taking from the blood some materials and giving up to it certain other materials. Now it is clear that its removal must make a permanent difference in the blood, and since the whole organism is remarkably sensitive to even slight changes in the composition of the circulating fluid, very marked results may well follow the removal of such organ. And this is in fact found experimentally to be the case.

It has long been known that extensive disease of the thyroid gland, a small reddish organ, weighing about one or two ounces, found at the front of the throat, is followed by extensive alterations in the nutrition of the body generally. The patient becomes swollen from the overloading of the connective tissues with a mucinous exudation; the nervous and muscular systems are seriously affected; the power of generating heat is greatly modified; and the final result is, in the first instance, the production of a condition of semi-idiocy, ultimately followed, if the disease be extensive, by death. Precisely similar results have been found in animals, and in fact in man as well, to follow the complete removal of this body. Yet the weight of this organ is not more than one sixteen-hundredth part of the whole weight of the body; and even this figure does not represent the enormous influence which a relatively small organ can exert upon the general nutrition of the body; for it is found that even if a minute part of the thyroid gland be left whilst the greater part is removed, the symptoms above enumerated do not supervene. Indeed, certain contradictory results which have been got by some observers after removal of the thyroid are explained by the fact that in some individuals there are minute detached particles of thyroid gland lying apart from the main organ; and that after the latter has been removed these detached particles may sufficiently carry on the function of the organ in relation to the blood and the nervous system to prevent the supervention of the deleterious symptoms which usually occur after its removal. Here is, then, a notable instance of the enormous influence exerted by a "next to nothing" upon the general organism.

Another illustration may be given from these ductless glands. It was noticed in 1849 by a celebrated physician, Dr. Addison of Guy's Hospital, that certain cases, accompanied by extreme debility, occurring in the human subject were associated with the appearance of peculiar bronzed patches on parts of the skin and mucous membranes; and on post-mortem examination of these cases, which always sooner or later have a fatal termination—and indeed sooner rather than later—he found the symptoms in question to be accompanied by disease and destruction of the supra-renal capsules—small bodies which are placed close to the kidneys, but which, so far as we know, have no physiological connection with them. Now when experiments came to be directed upon these bodies in order to elucidate their functions, and especially to observe whether their injury or removal was accompanied in animals also by symptoms similar to those occurring in man as the result of disease, it was found

by Brown-Séquard that when these bodies are totally removed in any animal the removal is speedily followed by a fatal result. These experiments of Brown-Séquard's were made in 1858, and at the time attracted some attention. They were repeated by other experimenters with similar effects. But some of those who removed the supra-renal capsules obtained contrary results, and for many years the matter remained in an undecided condition. It was even supposed that the fatal results which were got by Brown-Séquard might be due to the shock of the operation or to the fact that the removal necessarily involves certain parts of the sympathetic nervous system, and were not necessarily due to the removal of the supra-renals. Recently, however, attention has been again directed to the subject, and the experiment of Brown-Séquard has been repeated by Tizzoni (1889) and by Abelous and Langlois (1891 to 1894) in various animals, viz. frogs, guinea-pigs, rabbits, and dogs. I have myself performed two confirmatory experiments in monkeys. The result of all these recent observations is to show that the complete removal of the supra-renal capsules is not compatible with prolonged existence of life, and Abelous and Langlois have shown that it is accompanied by an alteration in the blood, which renders that fluid poisonous to other animals. The contrary results which have been obtained by some investigators are apparently due to the fact that in certain cases there are, as with the thyroid body, small isolated portions of supra-renal substance ("accessory capsules," as they are sometimes called) which have not been noticed and removed at the time of the operation, and that these small portions of supra-renal substance have served to maintain that proper relation between the blood and the gland which is sufficient to prevent the supervention of the symptoms in question.

Now the weight of both supra-renal capsules taken together is not more than three drachms, and their weight, as compared with that of the whole body, is only as 1 to 6000 or less. The accessory supra-renal capsules which may be left after the removal of the main bodies probably do not originally weigh more than one-twentieth of the whole structure, and yet this minute proportion of material (a material, so far as we know, unique in the organism) is nevertheless sufficient to maintain the composition of the blood and the nutritive equilibrium of the body, and thus to prevent the necessarily fatal result of complete removal.

Now it has been found in the case of the thyroid gland that patients in which this structure has been so diseased that its function is seriously interfered with, and animals in which it has been removed entirely, may be greatly benefited, if not indeed cured, by the inception, either subcutaneously or with food, of the thyroid glands of animals, or of the juice of such glands. Even where no affection of the thyroid can actually be detected, the exhibition of thyroid juice is frequently beneficial in certain conditions of the system, and it was noticed by Dr. Oliver, of Harrogate, that this is especially the case where there is a too marked constriction of the blood-vessels, the juice of this body tending in such cases to reduce the extreme tone of the vascular walls, which is the cause of this condition. Encouraged by this result, Dr. Oliver was led to examine the effects of other animal extracts, and among them that of extract of supra-renal capsule. The effect of this was precisely the reverse of that which he had got with the thyroid body, for he obtained evidence tending to show that in certain cases in man extract of supra-renal capsule can produce an *increase* of vascular tone and a diminution in the size of the arteries. Beyond this point, however, Dr. Oliver was unable to proceed by clinical experiment, and he accordingly came to my laboratory with the object of determining the precise physiological effect of the active substance of the capsules. The results which were obtained show that there is present in both alcoholic and watery extracts of the gland a most potent physiological substance which when injected into the body of an animal produces, even in minute doses, a remarkable effect upon certain parts of the nervous system, upon the muscular system, upon the heart, and upon the blood-vessels. If only as much as a grain by weight of supra-renal capsule be extracted with alcohol, and if this alcoholic extract be allowed to dry, and then be redissolved in a little water or salt solution and injected into the blood of a dog, the results which are obtained, considering the minute amount of substance added to the blood, are certainly most extraordinary. The nervous centre which regulates the action of the heart is powerfully affected, so that the heart either beats very slowly and weakly, or the auricles may even for a time stop beating altogether. If, however, these inhibitory influences be cut off by division of the vagi nerves, the effect of the poison upon the heart is of an opposite character. There is great acceleration of the rate of the beat and a great increase of force. This is accompanied by a strongly marked influence upon the blood-vessels, and especially upon the arterioles. The walls of these are chiefly muscular, and the drug exerts so powerful an action upon this muscular tissue as to cause the calibre of the vessels to be almost obliterated. The heart being thus increased in force and accelerated, and the calibre of the vessels almost obliterated, the result is to raise the pressure of the blood within the arterial system to an enormous extent, so that from a blood-pressure which would be sufficient to balance a column of some four inches of mercury the pressure may rise so high as to be equal to a column of mercury of twelve or more inches.

This result is obtained, as we have seen, by a very minute dose. We have to do here with a substance which is as potent, although in a different direction, as strychnia. Whether it is a useful substance formed by the supra-renals from materials furnished by the blood, and subsequently gradually used in the economy for the virtue of its action upon the circulatory system, or whether it is to be regarded as a poison, formed by the tissues during their activity and carried by the blood to the supra-renals, there to be rendered innocuous, we do not as yet certainly know. These are important points which must form the subject of further investigation. But, however this may be, it is clear that in this gland also we again meet with an instance of the physiological importance of what Sir Frederick Bramwell called the "next to nothing."

I will give one more instance, taken this time from a gland which is provided with a duct. Until quite recently it might have been thought that there was nothing very obscure regarding the functions of the pancreas. The pancreas is a digestive gland which lies below and behind the stomach: it has a duct which carries its secretion into the beginning of the intestine, and that secretion acts powerfully upon all constituents of the food, digesting starch, meat, and fat. It was not supposed that the pancreas had any other function to perform. Animals can live without this secretion, and to a large extent can continue to digest and absorb their food much as before; for it has been possible to divert the secretion from the intestine and to collect it at the surface of the body; and it is found under these circumstances that, although the food is not quite so readily digested, nevertheless the animal does not materially suffer from the lack of the secretion. It was discovered, however, a few years ago (by v. Mering and Minkowski) that if, instead of merely diverting its secretion, the pancreas is bodily removed, the metabolic processes of the organism, and especially the metabolism of carbohydrates, are entirely deranged, the result being the production of permanent diabetes. But if even a very small part of the gland is left within the body, the carbohydrate metabolism remains unaltered, and there is no diabetes. The small portion of the organ which has been allowed to remain (and which need not even be left in its proper place, but may be transplanted under the skin or elsewhere) is sufficient, by the exchanges which go on between it and the blood generally, to prevent those serious consequences to the composition of the blood, and the general constitution of the body which result from the complete removal of this organ. Now, some years ago it was noticed by Kühne and Sheridan Lea that, besides its proper secreting structure composed of tubular alveoli, lined by granule containing cells, there are highly vascular patches of peculiar epithelium-like cells scattered here and there in the substance of the pancreas, which are wholly unconnected with the ducts and, so far as one can judge, with the secretion of the gland. We do not know anything whatever about the function of these patches, although from their vascularity it is extremely probable that they are not without importance physiologically, and it is tempting to conjecture that it is these cells which are specially concerned in effecting that influence upon the metabolism of carbohydrates which experiment has shown to be peculiar to the pancreas.

The lesson to be drawn from these results is clear. There is no organ of the body, however small, however seemingly unimportant, which we can presume to neglect; for it may be, as with the supra-renal capsules, the thyroid gland, and the pancreas,

that the balance of assimilation and nutrition, upon the proper maintenance of which the health of the whole organism immediately depends, hinges upon the integrity of such obscure structures; and it is the maintenance of this balance which constitutes health, its disturbance, disease. Nor, on the other hand, dare we, as the investigation of the attraction-particle has shown, afford to disregard the most minute detail of structure of the body.

"All is concenter'd in a life intense,
Where not a beam, nor air, nor leaf is lost,
But hath a part of being."

PHYSICS AT THE BRITISH ASSOCIATION.

AFTER the President's address on Thursday morning, Lord Kelvin opened the proceedings in Section A with an account of some preliminary experiments made by himself and Mr. Maclean on the electrification of air by the subtraction of water from it. The subject is one in which Lord Kelvin has been for many years interested, and he commenced experimenting on it as far back as 1868. The nature of the results now obtained was illustrated by his insisting that the proper title of the paper was "Preliminary experiments *to find if* subtraction of water from air electrifies it" (and not as in the *Journal*—"Experiments *proving* the electrification of air"). In the present investigation a large U-tube was used. One branch of this was filled with pumice-stone soaked in sulphuric acid; the other was simply varnished inside and out. By means of a platinum wire touching the pumice, connection was made with a quadrant electrometer. A metal cylinder screened the tube from external influence. Air from an ordinary blow-pipe bellows was blown through the tube steadily for an hour; and the electrometer showed an electrification rising gradually to about nine volts positive. This shows that the passage of the air through the tube gave positive electricity to the acid, and therefore sent away the dried air electrified negatively. No such effect was observed when the pumice was moistened with water instead of sulphuric acid. The experiments are to be repeated with precautions to prevent any bubbling of the air through liquid in the tube; for it was observed that the strong positive electrification of the tube (when acid or calcium chloride was used) seemed to commence suddenly as soon as a gurgling sound, due to bubbling through free liquid, began to be heard. The authors have reversed the conditions, and have first dried air by passing it over sulphuric pumice, and then passed it through a tube containing moistened pumice. The tube became negatively electrified, but this may have been due to the negative electrification of the dry entering air. This experiment is to be repeated with dried and dis-electrified air. Lord Kelvin also described certain preliminary experiments made by himself and Mr. Galt with the object of comparing the discharge of a Leyden jar through different branches of a divided channel. The metallic part of the discharge channel was divided between two wires of conducting metal, each consisting in part of a test-wire. Each of the two test-wires consisted of 51 cm. of platinum wire of 0·006 cm. diameter and 12 ohms resistance stretched in a glass tube. One end was fixed to a solid brass mounting, and the other was attached to a fine spring carrying a light arm for multiplying the motion. The testing effect was the heat developed in the test-wire by the discharge, as shown by the elongation, the amount of which was measured by a tracing on sooted paper carried by a drum. The wires to be tested were generally of the same length. When they were of the same material but of different diameters, the testing elongation showed, as might be expected, that the test-wire in the branch containing the thicker wire was more heated than the other. With wires of various non-magnetic materials, of the same resistances but different lengths and diameters, the testing elongations were very nearly equal. In one experiment two equal copper wires were used, but one of them was coiled into a helix; the testing elongation in this branch was less than half of that in the straight branch. Lastly an iron wire was compared with a platinoid wire of equal resistance but greater diameter. The heating effect in the platinoid branch was nearly one-and-a-half times as great as in the iron branch. This is interesting in relation to Lodge's experiments on alternative paths, which were not decisive in showing any general superiority of copper over iron of the same steady ohmic resistance, but even showed a seeming superiority of the iron for efficiency in the discharge of a Leyden jar.

Prof. Oliver Lodge followed with a communication on "photo-electric leakage." It is known that a negative charge on an electrified surface escapes much more rapidly when the surface is illuminated with ultra-violet light (Hallwach's experiment). Prof. Lodge has investigated the rate of discharge for a number of substances under positive as well as negative electrification, and in hydrogen as well as air. He finds that when the inside of an electrified pewter-pot is illuminated, it does not leak; but when the edge is illuminated, it leaks rapidly. Thus the leakage appears to be a matter of surface-tension, and not of potential. In the discussion which followed, Prof. S. P. Thompson stated that he had verified the statement made by Elster and Geitl, that when the light is polarised the effect depends upon the plane of polarisation, the leakage being most rapid when the Fresnellian vibrations are in such a direction as to "chop into" the surface. He has found an analogous difference in the action on selenium cells.

Mr. G. H. Bryan presented the second part of his report on the present state of knowledge in thermodynamics. In a lengthy and valuable paper he discusses the limitations to the law of distribution of energy in the kinetic theory. He deals primarily with the so-called Boltzmann-Maxwell law of distribution of energy among the molecules of a gas, which law forms the basis of the kinetic theory of gases. One of the main points kept in view has been to show, as far as possible, where to draw the line between dynamical systems which do, and dynamical systems which do not satisfy the law in question. A great advance in the subject is due to the extension of the use of generalised co-ordinates, by which greater generality has been given to results, and the analysis has been much simplified, as a comparison of Boltzmann's early papers with modern writings abundantly testifies. A further simplification has been effected by the extensive use of the Jacobian notation in this report. The report is divided into three sections. In Section I. the law is regarded in the aspect of a general dynamical theorem without reference to any particular applications, and without taking into account the effect of collisions. Section II. treats of its application to a system of bodies colliding with one another indiscriminately, and partaking of the nature of gas molecules. Section III. deals briefly with certain researches relating to the connection between the Boltzmann-Maxwell law and the Theory of Probability, the Virial Equation, and the Second Law of Thermodynamics. With regard to non-colliding systems (Section I.), it may be asserted that a large portion of our progress has been made in, firstly, showing that Maxwell's demonstrations are faulty and unsatisfactory, and by subsequently discovering fresh methods of proof, which, while leading to the same general conclusions, show more clearly the limitations and conditions under which these conclusions hold good. Test cases of Maxwell's law are given, and also an account of Mr. Culverwell's criticism of the "decisive" test case by which Lord Kelvin claims to have effectually disposed of the law. It is urged that uniformity of nomenclature is most desirable in this as in other branches of science, and hence that some definite understanding should be agreed on as to what precisely constitutes the Boltzmann-Maxwell law. The following statements are recommended:—(1) That the distribution of a large number of molecules or other dynamical systems of the same or different kinds in which the coordinates (q) and momenta (p) of each system are so arranged that the number of systems in the neighbourhood of any given state is proportional to

$$e^{-hE}dp_1 \ . \ . \ . \ . \ . \ dp_n dq_1 \ . \ . \ . \ . \ . \ dq_n,$$

h being the same for all the kinds of molecules or systems, be called the Boltzmann-Maxwell distribution. (2) That the law which asserts the permanence of the Boltzmann-Maxwell distribution in any particular case be called the Boltzmann-Maxwell law. (3) That in future these names be not applied to any corollaries such as that which asserts the equality of the average value of the squares into which the kinetic energy can be split up. That corollary may be called Maxwell's law of partition of kinetic energy.

The proof of the Boltzmann-Maxwell law, and the assumptions involved in it, may now be regarded as fully satisfactory for gases whose molecules collide with one another to a certain extent at random, but in a medium in which the molecules can never escape from one another's influence the subject still presents very grave difficulties.

On Friday the Section held a joint meeting with Section G, at the headquarters of the latter in Keble Hall. The first sub-

ject of importance was a discussion on integrators, harmonic analysers and integraphs, and their application to physical and engineering problems. A number of these were exhibited, both models and working instruments, some of the latter being beautiful specimens of Swiss workmanship. For the discussion an hour and a half had been allowed: of this the opener, Prof. Henrici, occupied the greater part, but did so to the entire satisfaction of his audience. The subject had been discussed at previous meetings by Sir Frederick Bramwell at Brighton, and by the late Mr. Merrifield at Swansea; Prof. Hele Shaw had also read a paper on the subject before the Institute of Civil Engineers. The first planimeter was invented by a Bavarian engineer named Hermann. It was lost sight of, but was subsequently reinvented in 1825 and 1826, and from it our present planimeters are derived. Amsler invented his instrument in 1854, and first published an account of it in 1856. Planimeters may be classified in two ways. As Prof. Hele Shaw subsequently remarked, it is natural for an engineer to classify instruments with reference to their mechanical action, and thus planimeters may be divided into two classes, according as the wheel does or does not slip. Prof. Henrici prefers a classification depending upon the geometrical properties involved in the action of the instrument. A planimeter measures the area swept out by a line. The length of the line may either be fixed or variable. Again, a line in a plane may either move or turn. To obtain general areas we have a choice of two combinations (for only special areas could be traced, *e.g.* by a line moving parallel to itself). The first class of planimeters depends upon the motion of a line which can both turn and move parallel to itself, but which remains of fixed length. The line takes the form of a rod of fixed length, one end of which is jointed to another rod so as to move on a circle about a fixed point (the pole), while the other end is provided with a tracing-point to be moved around the figure whose area is to be evaluated. These planimeters can only be used to integrate around closed curves. It does not matter where the wheel is placed along the rod, but its axis must be parallel to the axis of the rod. This introduces one of the most serious difficulties with which the maker has to contend. In Amsler's planimeter the rod can only be used on one side, so that the error is always in the same direction; but an improved form was exhibited in which the rod can be used on both sides, so that this error is eliminated. Then there is the slipping error. Maxwell drew attention to this, and was the first to propose an instrument in which there was no slipping at all. There are a number of planimeters in which the wheel, instead of rolling on the paper, rolls on a prepared surface. There is always some resistance to the motion of the wheel and counters, and this increases the slipping. The error can be reduced to a minimum by diminishing as far as possible (1) the friction between the paper and the wheel (as by using a prepared surface); (2) the resistance to the motion of the wheel. In using the instrument we should also avoid getting the instrument in such a position that the wheel has to move much at right angles to its own plane, for then the friction and slipping error is greatest. Amsler, in his first paper (1856), foreshadowed many improvements which have since been carried out; and in his second paper (he only published two), he described a planimeter depending upon the action of a cylinder rolling on a sphere, in which there was no slipping. Maxwell suggested two forms of instrument in which slipping was altogether avoided; but they were never made. The second class of planimeters depends upon the motion of a line of variable length which moves without turning. They give the value of definite integrals between any fixed limits, and may be called integraphs. Instruments of this type have been devised by Lord Kelvin, Abdank-Abakanowitz, Vernon Boys, and Conradi. To engineers it is more important to be able to integrate a curve than an expression; and an integraph can give the integral of a curve as a curve. Lord Kelvin and Boys have shown how instruments may be made to integrate a differential equation. The idea of a harmonic analyser was given by Amsler in his first paper as early as 1856, but Lord Kelvin first actually constructed one. It has been of great service in analysing tidal motion; but it is bulky, and cannot be carried about. Prof. Henrici has devised two others, one of which will give five terms in the expansion according to Fourier's theorem of any curve. These analysers should prove of great use to engineers and electricians, *e.g.* in investigating the action of valve-gear and the behaviour of dynamos. In the discussion which followed, Prof. Hele Shaw drew attention to the Hatchet planimeter as a most simple and efficient workshop instrument. Prof. Boys explained why it was so much more difficult to construct an instrument for differentiating than for integrating. An automatic differentiator appeared at present to be an impossibility. A person can differentiate with a machine; but a machine cannot of itself well differentiate. It is of the very nature of an integraph to smooth over the irregularities of a curve; whereas a differentiator would exaggerate all the irregularities of a curve.

Mr. Arnulph Mallock followed with a note on the behaviour of a rotating cylinder in a steady current. Lord Kelvin was in his best British Association form when discussing the resistance experienced by solids moving through fluids. As the time approached for Mr. Hiram S. Maxim's paper on flight, the audience grew to dimensions most easily explained by supposing that an experimental demonstration in Keble Hall was expected.

After Friday, on account of the large number of papers, the Section had to split up into two or three departments sitting simultaneously (and continuously, without any luncheon interval). Only the more important physical papers can be noticed here. On Saturday, Prof. Osborne Reynolds described and illustrated experimentally the successive stages in the motion of water passing under gradually increasing pressure through a vertical tube constricted in the middle. At first the water leaves the constriction in the form of a narrow, steady jet. As the pressure increases it fills the lower part of the tube, and eddies appear below the constriction; but the motion is still steady. The third stage is that of turbulent motion. Finally, there is an appearance as of air-bubbles at the constriction, accompanied by a singing or hissing sound; the water is now boiling under diminished pressure. Prof. S. P. Langley gave an account of his recent researches on the infra-red spectrum to an audience most unwilling to allow him to stop, and rather impatient at the manner in which his lantern slides were exhibited. The President (Prof. Rücker) and Prof. Norman Lockyer heartily congratulated Prof. Langley on the magnificent success of his work, which will be fully described in a subsequent number of NATURE. Dr. E. Pringsheim followed with an account of his new determination of the ratio of the specific heats of certain gases.

The first paper on Monday was one by Dr. A. Schmidt, on a new analytical representation of terrestrial magnetism. Prof. Schuster followed with two papers: in one of these he examined a suggested explanation of the secular variation of terrestrial magnetism, and in the other he discussed the minimum current which could be observed in a galvanometer of given dimensions wound in various ways. Lord Rayleigh followed with three papers.

In the first of these he described experiments made by him to determine the minimum current audible in the telephone. The estimates previously put forward vary widely: Preece gives 6×10^{-13} ampere; Tait, 2×10^{-12}, and De la Rue 1×10^{-8} ampere. Ferraris is the only experimenter who has given satisfactory details of his experimental methods; he found that the current diminished when the frequency increased, and that a minimum current of 5×10^{-9} ampere was required at a frequency of 594. His experiments were made with a make-and-break apparatus, which would give higher harmonics in addition to the stated frequencies. In Lord Rayleigh's experiments electromotive forces of the harmonic type were produced by the revolution of a magnet in the neighbourhood of an inductor coil of known construction. The revolving magnet consisted of 2·5 cm. of clock-spring driven, windmill fashion, by air from an organ bellows. The magnetic moment of the magnet was deduced from observations with a magnetometer. The inductor coil was the one which had been used as the "suspended coil" in the determination of the electro-chemical equivalent of silver, and it was placed with its centre vertically below that of the magnet. From the known data the induced electromotive forces were calculated. The current was carried to a distant part of the house through leads, and was varied by introducing a resistance-box going up to 10,000 ohms; the adjustment of the sound could thus be made by the observer at the telephone. Theory shows that the minimum current required in a telephone should be inversely as the square root of the resistance. Two telephones of the Bell unipolar type were used: the data given below refer to one which had a resistance of 70 ohms. When the magnet was driven at full speed the frequency was 307, and

the minimum current observed was $3\cdot6 \times 10^{-7}$ amperes. In order to extend the determinations to higher frequencies, recourse was had to magnetised tuning-forks vibrating with known amplitudes. With a frequency of 512 the minimum current was $7\cdot0 \times 10^{-8}$, and with a frequency of 640 it was $4\cdot4 \times 10^{-8}$ amperes. Lord Rayleigh's second paper was on the quantitative theory of the telephone. About this so little is known that even an attempt to determine the order of magnitude of the physical quantities involved is of great value. The method adopted is to consider first the case of an infinitely long thin rod of iron, divided by a transverse gap, and encompassed by an infinite coaxial magnetising coil. He finds the force exerted across the gap by a periodic current, and then replaces one-half of the infinite rod by the plate of the telephone, and reduces the coil to the actual dimensions used in practice. The force in dynes exerted at the centre of the telephone plate is calculated to be equal to $1\cdot7 \times 10^{6}$ C., where C is the current in amperes. By actual experiment the force was found to be equal to $0\cdot6$ C. Experiment also showed that the displacement of the plate produced by a current C was C $\times$ 0·08 cm. The amplitude of the motion produced depends largely upon the relation between the frequency of the impressed vibration and those natural to the plate. For the telephone in question, assuming the plate to be clamped all round the edge, the frequency of the gravest symmetrical mode is calculated to be about 991. On making the plate speak on its own account, the frequency found was 896. As it is almost impossible to estimate the amplitude when the frequency of the force is near any of the free frequencies, the vibration number 256 is taken for calculation. At this pitch the minimum recorded current is $8\cdot3 \times 10^{-7}$ amperes; and the amplitude corresponding to this is $6\cdot8 \times 10^{-8}$ cm. Assuming the telephone to be applied closely to the ear, so as to include 20 c.c. of air, it is shown that the condensation (in atmospheres) produced is $1\cdot4 \times 10^{-8}$. For higher frequencies than 512 the actual sensitiveness, in virtue of resonance, is greater than the value calculated by the above method.

Prof. J. A. Ewing exhibited an apparatus for measuring small strains. The measurement of Young's modulus for considerable lengths of wires, as carried out in physical laboratories, is an easy matter; but engineers have to investigate the behaviour of short bars, and require an instrument which should be convenient and expeditious in use. In the instrument described these ends are achieved without any sacrifice of accuracy. There is only a slight mechanical magnification of the extension, but by means of a microscope forming part of the instrument, readings are made to 1/100,000th of an inch, and the readings are calibrated by a simple device which forms part of the instrument. If the arms have the same coefficient of expansion as the material of bar, there is automatic compensation for change of temperature. Difference readings were given for the extensions produced in a half-inch steel bar by twelve successive loads increasing each time by half a ton: these only varied between 10·4 and 10·7. The instrument is attached to the bar under examination in such a way as to measure strictly the *axial* elongation. It is well adapted for the investigation of small strains in parts of structures (*e.g.* members of railway bridges).

Mr. F. G. Baily made an important and interesting communication on hysteresis in iron and steel in a rotating magnetic field. It has long been known that, up to the limits of experiment, the value of hysteresis in an alternating magnetic field increases continuously. But it is a deduction from Prof. Ewing's molecular theory of magnetism that in a rotating magnetic field the hysteresis should diminish at a high induction, or at least show a reduction in the rate of increase. The following experiment substantiates this deduction in a very complete manner:—An electromagnet is rotated on bearings concentric with the bore of its own pole-pieces, which were bored out cylindrically. In the polar cavity a finely-laminated armature is suspended between centres, and held by a spiral spring attached to the axle and to a fixed support. Movement of the armature is indicated by a beam of light reflected from a mirror on it. On rotating the magnet, the armature tends to rotate with it by reason of hysteresis. The motion is checked by the spring, and the consequent deflection is proportional to the instantaneous value of the hysteresis per revolution. The curve of hysteresis and induction obtained commences like that in an alternating field, rising very slowly at first, then more rapidly, but finally reaching a maximum and bending over. The fall is very rapid so far as the experiments have been tried, shows no sign of becoming asymptotic, but runs straight towards the zero line. Soft iron and hard steel give the same results, the differences between them corresponding to their differences in the **B/H** curve. The three states of molecular arrangement, which are the essential point of the molecular theory, are exactly reproduced in the hysteresis curve. This first stage of quasi-elastic movement gives a very small hysteresis value. The second stage of irregular molecular groups and magnetic combinations gives a value approximately proportional to the induction at a steep inclination; this extends to the knee of the **B/H** curve. The third stage of approaching saturation gives a rapidly diminishing hysteresis when the molecular magnets are ranged in regular order along lines of force, and new combinations and irregular movements are prevented. Since the non-appearance of a correspondence between the **B/H** curve and the hysteresis curve in alternating fields has been urged as an argument against the molecular theory of magnetism, this complete accord and verification of the deduction previously made is important as giving powerful support to Prof. Ewing's theory.

Prof. S. P. Thompson briefly explained how he had verified the magnetic analogues of well-known propositions respecting optical images in plane mirrors. The experiments were made by placing a magnetic pole in front of a sheet of iron, and investigating the field by an exploring coil connected to a ballistic galvanometer. Prof. A. M. Mayer showed how beats and beat-tones could be produced by two vibrating bodies whose frequencies of vibration are so great as to surpass the limit of audibility. He has also employed the transverse vibration of bars at various temperatures to determine the variation of the modulus of elasticity with change of temperature.

On Tuesday morning there was a joint meeting with Section I, to discuss theories of vision. Prof. Oliver Lodge showed experiments to illustrate Maxwell's theory of light. Electromagnetic waves produced by a small vibrator were allowed to fall upon a detector placed inside a large copper "hat." The detector consisted of a glass tube containing iron borings forming part of a circuit with a galvanometer. On account of its mode of action, this detector is called by Prof. Lodge a "coherer." Under the action of the waves its resistance diminishes and the galvanometer current increases. The coherer was used to demonstrate the reflection, refraction, and polarisation of electromagnetic waves. The audience, which filled every part of the large museum lecture-room, repeatedly showed its warm appreciation of Prof. Lodge's beautiful experiments. His electrical theory of vision may be briefly described as a suggestion that light-waves do not directly produce the sensation of vision, but that their action (like that of the electromagnetic waves in these experiments) is a kind of "trigger" action.

In the subsequent Section-meeting, Principal Viriamu Jones gave the results of further determinations of resistance in absolute measure by the Lorenz method. The apparatus had previously been used to determine the absolute resistance of mercury, and has now (with modifications ensuring still greater accuracy) been employed to measure certain coils whose resistance in terms of the Cambridge Standards is known. He also exhibited a new form of standard coil of low resistance.

In the absence of Prof. J. J. Thomson, his paper on the velocity of the cathode rays was read by Prof. Fitzgerald. The phosphorescence shown by glass in the neighbourhood of the cathode was ascribed by Crookes to the impact of charged molecules driven off from the negative electrode. The remarkably interesting experiments of Hertz and Lenard, which show that thin films of metal interposed between the cathode and the walls of the tube do not entirely stop the phosphorescence, have led some physicists to doubt whether Crookes' explanation is the true one, and to regard the phosphorescence as being due to a kind of ultra-violet light. The view to which Lenard has been led by his experiments—that the cathode rays are ethereal waves—demands the most careful consideration; for if it is admitted, it follows that the ether must have a structure either in time or space. A magnet produces no effect upon ultra-violet light unless this is passing through a refracting substance. Now these cathode rays are deflected by a magnet, so that on the above view it must follow that in the ether in a magnetic field there must either be some length with which the wave-length of the cathode rays is comparable, or else some time comparable with the period of vibration of these rays. Prof.

Thomson first proved by experiment that a magnet acts on the cathode rays through the whole of their course, and does not merely affect the place on the cathode at which they have their origin. He then proceeded to investigate the velocity with which the cathode rays travel, for it seemed that a knowledge of this velocity would enable us to discriminate between two views as to their nature. If they are ethereal waves, we should expect them to have a velocity comparable with that of light; if they are caused by molecular streams, their velocity should be that of the molecules, which we should expect to be very much smaller than that of light. The value found for the velocity of the cathode rays was $1 \cdot 9 \times 10^7$ cm./sec., which is small compared with the velocity of the main discharge from the + to the − electrode. It is much greater than the velocity of mean square of the molecules; it agrees very nearly with the velocity which a negatively electrified atom of hydrogen would acquire under the influence of the potential fall which occurs at the cathode.

On Wednesday, M. Cornu exhibited some brilliant optical experiments illustrating Babinet's principle. Prof. W. Förster described the displacements of the rotational axis of the earth. His results had been deduced by investigating the results of 6000 determinations of latitude in various parts of the globe. The maximum amplitude amounts to nearly half a second, which corresponds to a motion of the pole amounting to 40 or 50 feet. It appears that we are now approaching a period of minimum amplitude.

CHEMISTRY AT THE BRITISH ASSOCIATION.

THE meeting of Section B at Oxford will be remembered as one of quite exceptional interest. That portion of the President's address urging upon the University fuller recognition of the claims of science in their curriculum will doubtless have valuable results. Certain of the communications brought forward in the course of the meeting may be described as epoch-making. The presence of a large number of eminent foreign chemists served further to render the proceedings memorable and attractive.

The Committee for preparing an International Standard for the Analysis of Iron and Steel reported that their work was now completed, and that it is proposed to shortly deposit the standards with the Board of Trade, or with some other suitable authority where they will be at the public service.

Prof. Clowes gave an account of his experiments on the proportions of carbonic acid in air which are extinctive to flame, and which are irrespirable. He finds that the flames of candles, oil, paraffin and alcohol are extinguished by air containing from 13 to 16 per cent. of carbonic acid. The flame of coal-gas, however, required the presence of at least 33 per cent. of the extinctive gas, and the flame of hydrogen was not extinguished until the amount of carbonic acid in the air reached 58 per cent. Comparing his experiments with those of Mr. J. R. Wilson on the percentage of carbonic acid required to suffocate a rabbit, Prof. Clowes concludes that air, containing at least 10 per cent. of carbonic acid more than is required to extinguish a candle flame, can be breathed with impunity. By taking advatage of the extraordinary vitality of the hydrogen flame in presence of high proportions of carbonic acid, it is possible to maintain the flame of the composite safety-lamp (previously described by the author), after the oil flame has been extinguished.

Mr. Haldane concluded from some experiments he has made on the respirability of air containing carbonic acid, that the percentage of this gas, considered by Prof. Clowes to be respirable, is too high.

Much interest was shown in the successful experiments of Dr. Lobry de Bruyn, demonstrating the properties of free hydroxylamine. On heating a small amount in a test-tube, a sharp explosion took place. Left exposed to air, it was shown by its action on iodised starch-paper to have become converted into nitrous acid. In a series of test tubes its behaviour with various reagents was demonstrated. With potassium permanganate, and with chromic acid oxidation took place, accompanied by flame; potassium bichromate produced an explosion. The anhydrous sulphates of copper and cobalt were reduced by the substance. Free hydroxylamine was dissolved in anhydrous ether, and sodium added, hydrogen was evolved and the very explosive sodium compound of hydroxylamine produced. Chlorine and iodine were shown to act spontaneously on hydroxylamine, producing inflammation. It is of interest that hydroxylamine will dissolve many salts which are soluble in water, the order of solubility differing in the two solvents.

Dr. Bernthstein described a new bacterium which occurs in milk, whose chief property is that of peptonising the caseine, thus forming a soluble compound, and rendering the milk transparent, and more readily digested.

On Friday a discussion took place on the behaviour of gases with regard to their electrification, and the influence of moisture on their combination. It was opened by Prof. J. J. Thomson, who exhibited some brilliant experiments illustrating the connection between chemical change and electrical discharge through gases. The gases were confined, under a pressure of about 100 mm. in glass bulbs which were placed in the centre of a coil of wire connecting the exterior of two Leyden jars, the interior coatings of which were connected with the two poles of a Wimshurst machine. As each spark passes between the poles of the machine, a rapidly alternating current is set up in the coil, and hence by induction in the gas. In the case of oxygen it was found that the moist gas gives a vivid incandescence, followed by an afterglow or phosphorescence, on removing the bulb from the coil. With the dry gas, on the other hand, incandescence does not take place. The incandescence, can however, be started in the dry gas by a brush discharge, and if once started continues under the influence of the induced current. With air the phenomenon is reversed; damp air does not glow, dry air will. By making use of two coils in one of which was a beaker of fairly strong sulphuric acid, and in the other a bulb containing moist oxygen, the presence of the acid was shown to prevent the incandescence in the bulb, showing the conductivity of the gas to be much greater than that of the acid. As the glow is only given in gases forming polymeric modifications, it is suggested by Prof. Thomson that the drops of water present may act as conductors causing the original molecules to dissociate. In the case of dry gases this preliminary dissociation can only be brought about by expenditure of a large amount of energy. Alcohol vapour will behave similarly to water, and it becomes of interest to study other solvents.

Mr. Brereton Baker followed with some experiments on the influence of moisture on chemical substances. He showed that ammonia and hydrochloric acid when dry do not combine. He also exhibited tubes containing dry sulphur trioxide and cupric oxide, and dry sulphur trioxide and lime, side by side without action upon one another, a kind of "chemical happy family," as he expressed it. He concludes that the function of moisture is physical rather than chemical from the fact that on heating together a dry mixture of cupric oxide, carbon and oxygen, no action takes place. He has obtained analogous results to Prof. Thomson, by using semi-vacuous tubes, into one end of each of which a platinum wire is fused and which contain a small quantity of mercury. On shaking these tubes in a dark room incandescence takes place in those containing moist oxygen. This is less if nitrogen is present, and ceases if the gas is dry. It was resolved in committee that Prof. Thomson's and Mr. Baker's papers should be published in full.

Dr. Ewan read a paper on the rate of oxidation of phosphorus, sulphur and aldehyde, a portion of which has already appeared in NATURE. The results obtained with aldehyde are free from the uncertainty produced by the correction for the changing rate of evaporation. When proper precautions are taken this reaction goes perfectly regularly at 20°, and its velocity is proportional to the product of the pressure of the aldehyde and the square root of the pressure of the oxygen. These results are most simply explained by assuming (in accordance with Williamson's theory), that the oxygen first splits up to some small extent into atoms, and that these alone take part in the oxidation.

In the discussion which followed the reading of these papers, Prof. Schuster spoke of the difficulty experienced in passing a discharge through mercury vapour.

Prof. Pringsheim noted the importance of ascertaining the relation between the spectrum given by the discharge, and that of the after-glow in the gas.

Mr. Vernon Harcourt remarked that Mr. Baker's results show that the part played by water in these reactions is probably unique, and is not shared by many, if any, other substances.

With reference to the dissociation of molecules, Mr. Baker

pointed out that the atoms of gases can change their mode of combination independently of water, *e.g.* in the decomposition of potassium chlorate, and in the combustion of carbon disulphide. He suggested that the residual glow is due to the reformation of the original molecules.

Prof. Fitzgerald drew attention to the high specific inductive capacity of water, and contended that Prof. Thomson's explanation of his results meant simply that in a large molecule the atoms can change places.

The President understood Prof. Thomson to hold that water was present in actual drops, and queried whether that could be so in the explosion of carbon monoxide, where a very high temperature is reached.

Prof. Thomson, in his reply, seemed of opinion that minute drops might be present even in the case mentioned by Prof. Dixon. In conclusion he urged the desirability of the preparation of pure compounds in the large laboratories of the country, which might be sent to the physicist for investigation.

Prof. Hartley then described some new methods of spectrum analysis and some Bessemer flame spectra. He has found that if a mixture containing alkalies and alkaline earths be fused with borax or silica, the alkalies are first volatilised and give their characteristic spectra very clearly. For obtaining spectra at high temperatures it has been found useful to heat the substance in the oxy-hydrogen flame on a rod of kainite, pipe-clay, or dried alumina. The elements can be classified according to the type of spectrum given under these conditions. On vapourising alloys, those constituents which, when free, give band spectra, are found to produce line spectra, *e.g.* silver, in an alloy of copper and silver. This is thought to be due to the difference between the vapour pressure of the element when alloyed, and when in the free state.

The spectrum of the Bessemer flame has been studied with special reference to the bands produced by manganese.

Mr. J. W. Thomas read a paper on the chemistry of coal formation, in which he endeavours to trace the connection between the properties of a coal and the character of the vegetation from which it probably originated.

On Monday a large audience assembled to hear the announcement by Lord Rayleigh and Prof. Ramsay of the existence of a new gas in the atmosphere. It appears that certain experiments of Cavendish pointed to the presence, in air, of some substance other than the gases with which we are familiar. Attention was recalled to this substance by the fact that the density of nitrogen obtained from atmospheric air differs by about ½ per cent. from the density of nitrogen obtained from other sources. It was found that if air (with excess of oxygen) be subjected to electric sparks, the resulting nitrous fumes absorbed by potash, and the excess of oxygen by alkaline pyrogallate, there remains a residue which is neither oxygen nor nitrogen, as can be seen from its spectrum. The same gas can be isolated by exposing nitrogen obtained from air to the action of magnesium. As the magnesium gradually absorbs the nitrogen, the density of the residual gas gradually rises to nearly 20. The newly discovered substance constitutes nearly 1 per cent. of the atmosphere, and gives a spectrum with a single blue line much more intense than a corresponding blue line in the nitrogen spectrum.

Sir H. E. Roscoe, in proposing a vote of congratulation on the discovery, spoke of the special interest which attached to it as being the outcome of a purely physical observation.

Prof. Emerson Reynolds noted the place which the new substance, if it proved to be an element, would occupy in Mendeléef's table among the platinum metals.

Prof. Roberts-Austen suggested that this gas might be the one which is frequently found as a residue among the gases extracted from steel.

The President, in putting the vote of congratulation to the meeting, drew attention to an observation made by Prof. Dewar, that while a mixture of pure liquefied oxygen and nitrogen forms a clear liquid, air in a similar state shows a turbidity. The President suggested that this turbidity might be due to the new gas.

The question is discussed by Prof. Dewar in a letter to the *Times* for August 16, in which he states that the substance causing turbidity does not amount to 1 per cent. of the whole liquid.

The next communication was by Prof. McLeod, on Schuler's yellow modification of arsenic. This is produced as a yellow sublimate when pure arsenic is heated in vacuo. The substance rapidly changes to the black modification.

Some very interesting experiments on the electrolysis of glass were described by Prof. Roberts-Austen. In conjunction with Mr. Stansfield he has found that if a bulb of soda-glass be filled with sodium amalgam and immersed in a vessel of mercury heated in a sand-bath to rather over 200°, on connecting the sodium amalgam and the mercury respectively with the terminals of a battery, sodium will pass from the amalgam through the glass into the mercury. At the end of the experiment the glass is unchanged. If lithium amalgam be substituted for the sodium amalgam, however, a certain percentage of lithium is found in the glass at the end of the experiment, sodium from the glass is driven into the mercury, and the glass is altered in appearance and frangibility. With potassium amalgam and soda-glass no change takes place. These phenomena are believed by Prof. Roberts-Austen to depend on the relative atomic weights and consequent atomic volumes of the elements concerned. Lithium, having a smaller atomic volume than sodium, is able to follow in the galleries left by the atoms of the latter metal; potassium, on the other hand, having an atomic volume greater than sodium, cannot force a passage. From the results he has obtained, using other amalgams, such as those of gold and copper and different kinds of glass, Prof. Roberts-Austen hopes to throw light on the formation of mineral veins in rocks which apparently have not undergone fusion.

Mr. J. W. Rodger gave an account of the experiments which have been conducted by Prof. Thorpe and himself on the relations between the viscosity of liquids and their chemical nature. The method adopted allowed a rapid succession of experiments to be made on the same liquid, at different temperatures. In the case of the fatty acids and alcohols examined, evidence has been found of the existence of molecular aggregates.

Dr. J. H. Gladstone described some experiments on the rate of progress of chemical change. The chief reaction investigated was that which takes place when platinum chloride and potassium iodide are mixed, resulting in the formation of the dark coloured iodide. This change begins rapidly, with no period of inertness or "reluctance." Its completion is much retarded, however, by the presence of potassium chloride. A change which does require time to attain a maximum rate is that which takes place when cuprous oxide is immersed in a solution of silver nitrate, the silver which is produced making its appearance only after some time.

A similar change to the latter was described by M. Paul Sabatier, in which litharge added to silver nitrate solution is converted into the puce-coloured oxide of lead, with simultaneous deposition of silver.

A paper was read by Mr. Vernon Harcourt, in the name of the late Mr. Percy B. Lewis, on a new and very delicate method for determining the freezing points of very dilute solutions.

Dr. M. Wildermann gave an account of experiments he had made with Mr. Lewis's apparatus, and said that they fully confirmed the predictions of the Van't Hoff Arrhenius theory.

Mr. W. W. Randall described his apparatus for measuring the colour-tint of dilute solutions. His experiments, instituted in order to determine whether dissociation takes place in dilute solutions, are of a qualitative character. At their commencement he was not aware of the careful spectrometric work of Dr. Ewan on the same subject.

Mr. Philip Hartog read a paper on the distinction between compounds and homogeneous mixtures, a portion of which recently appeared in a letter to NATURE. He showed that until lately there was no satisfactory experimental criterion for distinguishing easily between true compounds and such mixtures, but the recent work of Raoult showed that the freezing point of a pure compound was always lowered, and its boiling point raised, by any admixture.

Prof. J. A. Wanklyn's paper on new evidence as to the atomic weight of carbon was received with interest, though not with unanimous support, by the chemists present. By fractionating Russian petroleum the author has obtained hydrocarbons of constant boiling point, whose vapour densities point to their all containing carbon atoms of the weight 6.

Dr. J. B. Cohen described a simple form of apparatus for determining carbonic acid in the air, depending on the length of time required by the carbonic acid in a given volume of air to neutralise a known amount of standard lime solution insufficient to combine with all the carbonic acid present.

Mr. A. P. Laurie contributed a paper on "The Diffusion of very Dilute Solutions of Chlorine and Iodine." The interest-

ing result has been obtained that these elements in solution follow the law of gaseous diffusion, the chlorine diffusing twice as fast as the iodine.

Prof. J. W. Brühl gave an account of his investigations on tautomerism. By determining the molecular dispersion of compounds, he has been able to obtain values which are independent of temperature, and thus he has arrived at a sure means of distinguishing between bodies containing the group $HC—C=O$, or "keto" bodies, and those containing the group $C=C(OH)$, which he termed "enole" compounds. With simple ketones and di-ketones no tautomerism or change from the keto to the enole form was found to occur. Nor did it occur with the alkyl derivatives of ketonic acids; when, however, the alkyl was replaced by an acid radical in these cases, tautomerism occurred. The author had investigated derivatives of camphocarboxylic and of malonic and succinic acids, and found the above rule to hold good in these cases, although enolisation was found to depend not only on the number of negative groups present, but also on the position of these in the molecule, and on the simultaneous presence of alkyl groups, which latter sometimes rendered the molecule more stable. An interesting compound had been obtained, namely, mono-brom-formyl camphor, which was a true ketone, and which was the only compound known in which the keto form of the formyl radical was present. In conclusion the assumption of Lahr, that ketonic compounds possess a "labile" constitution, was shown to be untenable, no continuous internal atomic movement being probable. In the case of benzene derivatives, on the other hand, such changes probably occur, and are termed by the author "phasotropic."

Prof. E. Noelting read two papers entitled, respectively, "On Di-nitros Derivatives of the Aromatic Series," and "On the Formation of Indazoles from Diazo-compounds." Both papers dealt with compounds, which showed the dependence of stability on molecular grouping.

Dr. Caro described the method of obtaining a new rhodamine, or pink colouring matter, by the interaction of chloral hydrate and an alkyl derivative of metamidophenol. A salt of a leuco base is formed, which latter on oxidation by ferric chloride gave a blue colouring matter. It was shown by experiment that on heating a solution of this blue compound in water it turned to a fine pink, owing to an intra-molecular change.

A paper followed, by Drs. G. G. Henderson and A. R. Ewing, on "Tetrarsenites." The sodium salt, which was prepared by adding arsenious oxide to acid sodium tartrate, was easily soluble in water, and might be conveniently used for hypodermic injections of arsenic. Other salts had been prepared, and also a solution which probably contained the hypothetical tartrarsenious acid from which they were derived.

Dr. J. B. Cohen read a paper on "The Constitution of the Acid Amides," in which he showed that these might be divided into two classes—those which formed compounds with silver and crystallised in needles or prisms, and those which did not form silver compounds and crystallised in plates. To account for these differences he fell back on Hantzsch's theory of the stereo-isomerism of nitrogen compounds, and concluded that the amides contain a hydroxyl group.

A short discussion followed the paper, in which Prof. Dunstan quoted experiments which he had made on the action of trichloride of phosphorus on acetamide, which did not bear out Dr. Cohen's view of the constitution of the latter body.

Dr. Caro, however, did not consider Prof. Dunstan's experiment conclusive.

The report of the Committee on Isomeric Naphthalene Derivatives was read. Work had been done on chlor-sulphonic and brom-sulphonic derivatives of naphthalene, and the results tended to confirm the previous conclusions of the investigators.

The report of the Committee on the Action of Light upon Dyed Colours was read by the secretary, Prof. Hummel. The colours experimented with this year were chiefly yellows. Of these by far the largest number, ranging from "moderately fast" to "very fast," were to be found among the azo colours. The azoxy colours give good fast tints upon silk and cotton. The fastness of alizarin orange is probably greater than that exhibited by most other colours of the alizarin group. Very few fast yellows are derived from the natural colouring matters. The cultivation of weld, which yields the only fast and, at the same time, bright, natural yellows, is being gradually given up. It is fortunate then that efficient substitutes can be obtained from coal-tar, which, contrary to popular opinion, is the source from which the greatest number of colours fast to light are derived at the present time.

Dr. W. Meyerhoffer read a paper on "Certain Phenomena of Equilibrium during the Evaporation of Salt Solutions." For a given mixture of salts in a saturated solution it was found that there existed a certain transition temperature above which double decomposition took place. Thus with a saturated solution containing ammonium chloride and sodium nitrate, sodium chloride was formed above 6° C., while below that temperature no change took place.

GEOLOGY AT THE BRITISH ASSOCIATION.

OF the forty-three papers presented to Section C this year, comparatively few are of lasting importance, geologists having apparently saved up their best work for presentation at Zurich, or else having exhausted themselves at the excellent and successful session of the previous year. The President's address, containing an excellent epitome of the recent progress of mineralogy, was rather fitted for quiet and thoughtful perusal than for reading to a mixed audience, but it will be looked back upon as one of the most valuable of the contributions to the forthcoming volume of *Proceedings*. It was followed up by only one paper dealing with pure mineralogy, that of Mr. H. A. Miers, on a new method of measuring crystals. The two fundamental laws of crystallography—namely, (1) the constancy of the angle in crystals of the same substance, and (2) the law of simple rational indices—seem to be violated by those crystals which are liable to irregular variations in their angles, or those which have the simple faces replaced by complicated "vicinal" planes. Both these anomalies are exhibited by potash- and ammonia-alum. Brilliant and apparently perfect octahedra of these salts show large variations in the octahedron angle; other crystals show low vicinal planes in place of the octahedron faces. If it be true, as is supposed, that the octahedron angle varies in different crystals, it would be interesting to ascertain whether progressive variations can be traced during the growth of a single crystal, and whether some or all of the octahedron faces change their direction in space if the crystal be held fixed during growth.

In order to solve this problem a new goniometer has been constructed, in which the crystal is fixed at the lower end of a vertical axis, so that it can be immersed in a liquid during measurement. This device is in reality an inversion of the ordinary goniometer with horizontal disc; the liquid is contained in a rectangular glass trough with parallel-plate sides; one side is placed rigidly perpendicular to the fixed collimator, and the other is perpendicular to the telescope, which is set at 90° to the collimator. The trough is supported on a table which can be raised and lowered, so that the crystal can be placed at any required depth in the liquid. If the liquid used be its own concentrated solution the crystal can be measured during growth, and the changes of angle, if any, can be observed at different stages. In order that it may be held rigidly, the crystal is mounted, when small, in a platinum clip, which it envelops as it grows larger.

The results derived from the measurement of a large number of alum crystals are as follows:—

(1) The faces of the regular octahedron are never developed upon alum growing from aqueous solution.

(2) The reflecting planes (which are often very perfect) are those of a very flat triangular pyramid (triakis octahedron) which overlies each octahedron face.

(3) The three faces of this triangular pyramid may be very unequal in size.

(4) The triakis octahedron which replaces one octahedron may be different from that which replaces another octahedron face upon the same crystal.

(5) During the growth of the crystal the reflecting planes change their mutual inclinations; the triakis octahedron becomes in general more acute, *i.e.* deviates further from the octahedron which it replaces, as the crystal grows.

(6) This change takes place not continuously, but *per saltum*, each reflecting plane becoming replaced by another which is inclined at a small angle (generally about three minutes) to it.

(7) During growth the faces are always those of triakis octahedra; if, owing to rise of temperature, re-solution begins to take place, faces of icositetrahedra are developed.

These observations prove that the growth of an alum crystal

expresses an ever-changing condition of equilibrium between the crystal and the mother liquor. It does not take place by the deposition of parallel plane layers; new faces are constantly developed: since these succeed one another *per saltum* they doubtless obey the law of rational indices, though not that of *simple* rational indices. From the mutual inclinations of these vicinal faces it is possible to calculate with absolute accuracy the angle of the faces to which they symmetrically approximate. This angle is found to be that of the regular octahedron 70° 31¾'. The octahedron angle of alum is not, therefore, as appeared from the observations of Pfaff and Brauns, subject to any variation.

Mr. Howard Fox described a remarkable rock which occurs at Dinas Head in Cornwall, between a greenstone and a slate, and apparently intruded upon by the former. It has the composition of albite felspar, with as much as 10 per cent. of soda, and is like the keratophyres in composition as well as in the possession of concretionary and spherulitic structures. The nodules and spherulites stand out as the rock weathers, and the latter are shown by the microscope to consist of blades of albite radiating round centres of cryptocrystalline material. On the other hand, the rock might belong to the altered sediments called adinoles, of which some, in the Harz, yield 7.5 per cent. of soda, and with this the field evidence and the presence of idiomorphic crystals of ferriferous carbonate appear to agree. Mr. W. W. Watts exhibited photographs of a stack of Keuper sandstone at the Peakstones, near Alton, Staffordshire, which, he claimed to have proved, owed its resisting power to the existence of almost vertical planes in the rock cemented by the deposit of barium sulphate. These planes strike along a prominent ridge between two valleys, and at the end of it is the projection of the Peakstones rock. Other cases in which basement beds of the Keuper sandstone are similarly cemented were quoted by the author.

Amongst the papers dealing with Oxfordshire geology, that by Prof. Green demands attention first. In it he described the sections displayed at Fawler and Stonesfield, Shotover, Faringdon, Culham, and Swindon. The thinness of the Upper Lias at Fawler was remarked upon, and a curious case of contemporaneous erosion in the Forest Marble described; the peculiar character of the iron-sands was explained by their having been deposited in a long strait, in which Faringdon was a sheltered bay, suited for the growth of the organisms which here make up almost the whole deposit. In the section at Culham, which shows Gault resting directly on Kimmeridge or Portland limestone, the denudation of the iron-sand was described as a local phenomenon, it being found in full force at another section hard by. The excavations at Stonesfield, carried out by Mr. Walford, were the subject of a report by him, in which he showed that about 30 feet of limestone with clay seams, presenting on the whole the aspect of the great oolite, occurred beneath the "slate" bed. He intends to continue his excavations in order to determine the relationship of these deposits to the Chipping Norton limestone and the Clypeus grit of the Oxfordshire Inferior Oolite. In another paper the same author points out that the terraced hill slopes occur in one geological line in Oxfordshire, the outcrop of a band of micaceous marl in the Middle Lias just below the "red rock bed." The water penetrates from above where the Upper Lias has been stripped off by denudation and filters through to the top of the clay of the *margaritatus* zone, where it makes its escape. The saturated marls are continually creeping down hill, and, in doing so, give rise to the terraces.

Prof. W. Boyd Dawkins endeavoured to trace the submerged folds of palæozoic rocks under the mantle of newer formations in Oxfordshire, by means of the principle originally laid down by Godwin-Austen and elaborated by Bertrand in recent papers, that the great pre-carboniferous folds form lines of weakness, along which the upper skin of later rock wrinkles and cracks. The northern rim of the South Wales syncline, which contains the coal-basin, was traced eastwards through the Forest of Dean, the partially covered fields north of the Mendips, through Gloucester, Blenheim, Kirtlington, Quainton, Bishop's Stortford, Braintree, and Colchester. From this it is reasonable to infer that coalfields will be found in the area between this line and that from the Mendips to Hythe. One such the author claims to have been discovered at Burford, and he advises that further investigation should be carried on, there and in the neighbourhood, to set at rest the question whether workable coals occur in this syncline. Three other papers by the same author dealt with evidence from borings. The first drew attention to a seam of oolitic iron ore (grains of hydrated oxide embedded in calcium and iron carbonates) of Kimmeridgian age, met with at the Dover boring. The second dealt with the Permian strata of the north of the Isle of Man, consisting of 913 feet of red Roth-liegende sandstone, followed by 455 feet of calcareous conglomerates and breccias, which were correlated with the magnesian limestone; these rocks form a connecting link between those of Cumberland and North Ireland. The third communication described three borings in the north of the same island, one of which had penetrated 33 feet of salt-bearing marls apparently at a greater depth than 450 feet from the surfaces: the Triassic sandstones and saliferous marls again present another link between Carrickfergus and the English coast. A paper by Prof. Bonney instituted a comparison between the pebbles in the trias at Cannock and Budleigh Salterton, as a result of which he concluded that those of Budleigh must have come from the south-west, but that similar fragmental rocks fringed the ancient western land in localities far apart.

Sir Archibald Geikie corroborated Heddle's identification of the Pitchstone of Hysgeir, an island about eighteen miles west of Eigg, with that of the latter island. This lava, which flowed into a river channel sloping from east to west, is exactly like that of the Scuir of Eigg, but unfortunately its base cannot be seen. The old river channel would be at least 20 miles long, with a fall of perhaps as much as 35 feet in a mile. Gravels containing masses of volcanic rocks (some of them possibly thrown direct from volcanoes), with water-worn blocks of Torridon sandstone, grit, quartzite, and other rocks, rapidly thinning out or passing into fine tuff or volcanic mudstone, are to be found in the islands to the north of Hysgeir, intercalated at various horizons in the bedded basalts, and have doubtless been formed by the flooding and torrential action of contemporaneous rivers.

A joint discussion on the plateau implements of Kent, held by Sections C and H, was opened by Prof. Rupert Jones, who agreed with Prof. Prestwich that the implements were of human origin and dated back to an ancient time when the physical geography of the Weald was very different from its character to-day. Mr. Whitaker followed with a paper, in which he stated that he did not consider the plateau gravels so ancient as had been supposed by other authors, but that part of the deposit, at any rate, was a residuum which had grown where it stood, and is still growing, so that implements in it might be of almost any age. He further stated that he could find no evidence to connect men with preglacial or even glacial times. Amongst the other speakers were Mr. Montgomerie Bell, Sir John Evans, Dr. H. Hicks, Prof. Boyd Dawkins, General Pitt-Rivers, Sir Henry Howorth, Mr. Clement Reed, and Lieut.-Colonel Godwin-Austen.

The more important palæontological communications included Prof. Rupert Jones's eleventh report on Palæozoic Phyllopoda, Mr. Laurie's second report on the Eurypterids of the Pentlands; recording the obtaining of a large amount of material from which important results are to be expected; Dr. Traquair's preliminary notice of a new fossil fish from the upper Old Red Sandstone of Elginshire; Mr. Jeffs' descriptions of forms of Saurian footprints from the Cheshire Trias, some of which were new, while apparently none of them could be referred to any known species of Labyrinthodont; and Dr. Hicks's conclusion that the original home of the earliest forms of animal life was at some point in the Atlantic. Mr. Montagu Browne's third paper on Rhætic Vertebrata, in which teeth of "Saurichthyan" type were described in the same jaws as Labyrinthodont teeth, proving that the remains attributed to Saurichthys must be assigned to Labyrinthodonts, Plesiosaurus, Hybodus, and Gyrolepis, also called attention to remains of Rysosteus, Metoposaurus or Trematosaurus (?), and Dinosauria.

The consecration of Monday to pleistocene geology has almost become an institution, probably because, although new facts of consequence may not have been discovered within the year, at least one new interpretation of them, or a new theory founded on some of them, can always be relied upon. The day was opened by Mr. Bell's report on the well and borings at Chapelhall, near Airdrie, which completely proved that there was now no shelly clay to be found in the well or in borings in its immediate neighbourhood. Mr. Kendall followed with a report on the boulders examined and collected during the year.

Dr. Hicks next endeavoured to prove that the stratified gravels, sands, and clay of the plateaux of Hendon, Finchley, and Whetstone, which are covered by chalky boulder clay, had

been formed in a lake dammed to the east and west by ice in a moraine, a view which elicited considerable comment and disagreement from a number of glacialists present. Prof. Blake described areas in the Harlech Mountains, some of which were floored by bare, ice-scratched rocks, side by side with others covered by thick drift deposits. Prof. Bonney, judging by the temperature at which glaciers now form in the Alps, came to the conclusion that a fall of 15° to 20° F. would be quite sufficient to cause all the known glaciation in the northern hemisphere. Mr. E. P. Culverwell read a paper entitled "An Examination of Croll's and Ball's Theory of Ice Ages and Genial Ages," in which he stated that an appeal to figures conclusively proved the inadequacy of the astronomical theory as at present formulated. By calculating the comparative solar heat of the high eccentricity winter of 199 days, and that of 199 coldest days of the present winter, he showed that the isochimenals would be about 4° of latitude further south, and the isotherals more than 4° further north, then than now. In the "genial age" the shifting would not be more than $2\frac{1}{2}$°. This cause he considers totally inadequate to account for glacial and genial ages, and therefore falls back on changes in physical geography for the former, and shift of the pole, or greater solar and terrestrial activity, for the latter. In the discussion Sir Robert Ball defended his position, but was further attacked by Prof. Fitzgerald and other speakers.

Prof. Blake endeavoured to show that by the lowering of its centre of gravity as a whole the further end of an ice-sheet might be raised and carry boulders and detritus up a slope. The Rev. E. Jones presented the final report on the Elbolton Cave, stating that no palæolithic remains had been found, and that the investigation was consequently abandoned. The preliminary work, however, in the Calf Hole Cave, near Skipton, was more hopeful, and already a hafted implement, made of a chisel-like tooth bedded in antler and mounted in wood, had been found. In a short paper on the palæolithic section at Wolvercote, near Oxford, Mr. Montgomerie Bell described the section as follows: (1) a "northern drift" and subangular gravel resting in eroded hollows of the Oxford clay. (2) A river gravel containing shell seams, and in whose lowest bed palæolithic implements associated with mammoth, *Cervus elaphus*, *Bison priscus*, &c., have been found. (3) A bed of peat containing the remains of local plants. (4) Sand, mud, and hail close the section. Two palæolithic implements were recorded by Mr. Bruce Foote from an old alluvium, through which the Sabarmati river in Guzerat had cut a channel, varying from 100 to 200 feet in depth.

Dealing with questions of physical geology, Mr. Lobley tried to show that the contraction theory would not explain volcanoes and earthquakes. Dr. Tempest Anderson described three types of subsidences occurring in connection with volcanic rocks in Iceland; the most common type was due to a falling crust where the inner lava stream escaped, others were due to earthquakes, and still others to faulting, probably caused by subsidence of a volcanic centre as a whole. Dr. Johnston-Lavis recorded his observations on the activity of Vesuvius during the year. Mr. De Rance reported in underground waters. Prof. Herdman drew attention to the geological results flowing from his investigation of the bed of the Irish Sea. Prof. Sollas announced that arrangements were in progress for commencing the boring of a coral island. Prof. Hennessy described the channels of streamlets in estuaries as possessing a section constructed of two catenary curves, the only shape which gives a constant velocity whatever the depth of the stream; and Sir H. Howorth passed a series of strictures on current geological nomenclature, with suggestions for its revision, which will doubtless receive the attention due to so high an authority in these matters.

Mr. C. Davison's report on earth tremors contained an account of the trial and modifications of Mr. H. Darwin's bifilar pendulum, and of the horizontal pendulum used at Nicolaiew; then followed an elaborate analysis of the pulsations of the Greek earthquake of this year, showing how they spread to one observatory after another, and were felt at Rome, Siena, Nicolaiew, Potsdam, Kew, and Birmingham. In his report on geological photographs, Mr. Jeffs stated that the collection now amounted to 1055, and that the time seemed to have arrived when it should be housed in some convenient and central position, although it was still necessary to add to the collection, so as to make it thoroughly representative of the whole country. A number of photographs were exhibited and slides from some of them displayed at the second conversazione.

NOTES.

M. Gustave Cotteau, a Correspondent of the Paris Academy, in the Section d'Anatomie et Zoologie, died at Paris on the 10th inst.

Mr. M. A. Ryerson has presented to the University of Chicago the Ryerson Physical Laboratory, built at a cost of 250,000 dollars.

The International Congress of Applied Chemistry, which has just finished a session at Brussels, will hold its next meeting at Paris in 1896.

Dr. D. F. Oltmanns has been appointed Extraordinary Professor of Botany at the University of Freiburg-i. B.

The Imperial Acclimatisation Society of Moscow has founded a botanical section for the purpose of collecting materials for a Flora of Russia. The co-operation is invited of all who are able to assist in this work. Communications should be addressed to the Director of the Polytechnic Museum, Moscow.

Mr. H. W. Unthank informs us that while shore-hunting at Brightlingsea on August 4, he came upon a stranded Aurelia which exhibited a pentamerous instead of the usual tetramerous symmetry. The specimen is at present in the Brightlingsea Marine Laboratory of the Essex County Council.

Mr. Fred N. Scott, Assistant Professor of Rhetoric in the University of Michigan, has issued, in the form of a leaflet, a series of questions on the psychology of usage. He wishes to ascertain the origin of dislikes, especially of arbitrary, unreasoning dislikes, for certain words and phrases. He will be glad to send a copy to anyone who is interested in the subject and who will take the trouble to answer the questions.

The annual general meeting of the Federated Institution of Mining Engineers will be held in Newcastle-upon-Tyne, on Wednesday, September 5, in the Wood Memorial Hall of the North of England Institute of Mining and Mechanical Engineers. The papers down for reading are:—"The Stetefeldt Furnace," by Mr. C. A. Stetefeldt; "Walling and Sinking simultaneously with the Galloway Scaffold," by Mr. John Morison; "Timber Bridges and Viaducts," by Mr. Morgan W. Davies; "Explosions in Nova Scotian Coal-mines," by Mr. Edwin Gilpin, jun.; and "The Shaw Gas-tester for detecting the Presence and Percentages of Fire-damp and Choke-damp in Coal-mines, &c.," by Mr. Joseph R. Wilson. There will also be discussions on other papers, and various excursions.

The cholera epidemic is slowly spreading, especially in European Russia. Since the end of June, fifteen new districts in Russia have been declared to be infected, making a total of about forty. In Austria-Hungary, in many towns situate on the Vistula, and in Belgium, the disease has extended, and a number of fatal cases have occurred in places in Northern Holland. The Local Government Board is keeping a close watch on the progress of the epidemic, and every precaution is being taken to prevent it from obtaining a foothold in this country. Some anxiety will be felt for a month or so, for during this period the risk of infection is greatest. The dismal weather we have been experiencing this summer, though hardly conducive to pleasant holidays, has one redeeming feature, for it is decidedly unfavourable to the development of a cholera epidemic.

By the death of Dr. C. R. Alder Wright, at the end of last month, science lost a tireless and enthusiastic worker. He was educated at the Owens College, Manchester, and early showed an aptitude for scientific research. His work extended over a large part of the domain of chemistry. It comprises, says the *Chemical News*, "investigations of simple substances, like hydriodic acid, and some of the most complex substances, like

the vegeto-alkaloids, upon which he laboured for many years—sometimes alone, and sometimes in conjunction with Matthiessen and others. These researches deserve to be classical if any researches do, and they occupy an entire department of organic chemistry." Another subject to which Dr. Wright devoted great attention was the determination of chemical affinity in terms of electromotive force. The versatility of his genius is shown by the different subjects of his numerous papers. Isomeric terpenes, the smelting of iron in blast-furnaces, some points in chemical dynamics, and certain voltaic combinations, are a few of the subjects taken up by him. In 1889 he commenced a series of valuable communications to the Royal Society, on "Ternary Alloys," and the seventh part of the series was read before the Society at the beginning of last year. Thorough and conscientious in everything he undertook, Dr. Wright will always be regarded with esteem throughout the world of science.

THE Paris Société d'Encouragement pour l'Industrie Nationale is constantly giving evidence of its usefulness. The latest proof of this is to be found in the publication of a circular on the "Unification des Filetages et des Jauges de Tréfilerie." The Society has had the question of a standard screw-pitch and wire-gauge in hand since the end of 1891. It was, however, only three months ago that a meeting was held for the purpose of discussing the various systems suggested, and at that meeting the scales expounded in the pamphlet before us were accepted. To secure uniformity in the manufacture of screw-threads, the Society proposes a system, to be known as the "Système Français," in which the pitch increases in steps of half a millimetre, from a screw having a diameter of six millimetres and a pitch of one millimetre (No. 0) to one having a diameter of 148 millimetres and a pitch of 10·5 millimetres (No. 19). The formula which, in this system, allows the diameter (D) corresponding to any pitch to be deduced is $D = p(p + 8) - 1·5 \div 1·3$, where p is any pitch expressed on the adopted scale. By means of this, it is possible to extend the new system to screws of any diameter, and the number of the screw of which the diameter had been thus determined would be equal to the number of half-millimetres expressing the pitch, *minus* two. For screws less than six millimetres in diameter the standard drawn up by M. Thury for the Société des Arts of Geneva is adopted. In addition to promulgating these standards of screw construction, the Société d'Encouragement has developed a decimal system of gauging wires. In this system, the numbers of the wires express their diameters in tenths of millimetres; thus, a No. 7 wire has a diameter of 0·7 mm. Such a method of designating wires is as simple as it is scientific. In France and other countries using metric standards, the introduction of the new system will be comparatively easy; but we regret to say that in this land of complicated weights and measures, there is little hope of its adoption.

THE general sitting of the German and Austrian Alpenverein was held this year in Munich from August 8 to 11. At the end of the Congress several tours were conducted into the Bavarian and Austrian highlands. The Congress was attended by more than 6000 members, by delegates representing Alpine clubs in other countries, by Bavarian State delegates, and others. Business matters were smoothly arranged, the Central Committee showing an annual income of £9850, and in addition £5000 in hand. There are now 214 local sections of the Alpenverein, each of which pays a certain portion of the fees of membership to the Central Committee and retains the rest for independent income and outlay. Out of an unostentatious beginning, twenty-five years ago, has grown a club with a membership of over 31,000, and a many-sided activity and vigour of life promising a still greater future. The object of the Verein is to improve travelling in the Alps, and to increase our knowledge of them. Again, the management of the guides is entirely conducted by the Verein; the guides being occasionally placed for a short course of training in some central town. The Munich section is the largest of the Alpenverein sections, and was therefore well able to entertain the members generously. The halls in which the great festival of the Congress was held on Thursday, the 9th inst., were decorated on a magnificent scale. An exhibition of maps and models was held in the Academy of Sciences. The relief of the Jungfrau Group, by Simon, modelled to a scale of 1:10,000, attracted great attention. The surest proof of the success of the Alpenverein in its efforts for Alpine travelling is its popularity among the mountain inhabitants. They honour it for the good it has done their wild country and for the intercourse it has opened up. Meanwhile the Alpenverein plays its part worthily in the wider arena of science as journalist, cartographer, meteorologist, geologist, botanist, and all in no small measure.

DR. P. MIQUEL contributes to the *Diatomiste* an important paper on the re-establishment of the size of diatoms. This takes place, according to this observer, mainly by the activity of the nucleus. The protoplasm within a micro-frustule clothes itself with a thick extensible membrane, frees itself from the valves which imprison it, attains the normal size, and then secretes a siliceous envelope. No process analogous to conjugation could be detected by Dr. Miquel in a vast number of observations on many different species. He proposes the abolition of the terms auxospore and sporangial frustule, as not expressing accurately any process which actually takes place in the multiplication of diatoms.

THE Department of Science and Art has issued the following list of successful candidates for National Scholarships, Royal Exhibitions, and Free Studentships, awarded upon the results of the May Examinations this year. National Scholarships for Mechanics—Arthur H. Barker, 23, fitter, Pontefract; Edward R. Amor, 18, engineer apprentice, Devonport; John T. Rieley, 25, science teacher, Birmingham; Charles B. Brodigan, 24, draughtsman, London. National Scholarships for Chemistry and Physics—Charles A. West, 24, teacher, Tottenham, Middlesex; William H. White, 15, student, Ipswich; Harry Dean, 17, student, Manchester; William C. Reynolds, 24, pharmacist, London; John Lister, 18, student, Stockport. National Scholarships for Biological subjects—Ernest Smith, 25, assistant teacher, Huddersfield; Frank H. Probert, 17, student, London. National Scholarships—George M. Russell, 22, shipwright apprentice, Portsmouth; Samuel Stansfield, 21, engineering student, Todmorden; Gilbert T. Morgan, 23, chemist, Huddersfield; William G. Hall, 18, student, Nottingham; Arthur S. Cox, 17, student, Southampton; Frederick T. Munton, 25, joiner, Derby; George Wilson, 22, mechanical engineer, Sheffield; George E. Ashforth, 16, student, Manchester; George L. Overton, 19, watchmaker, Bradford, Yorks; Norton Baron, 21, engineering student, Ulceby, Lincs.; Ernest E. L. Dixon, 18, student, London. Royal Exhibitions—Frank Fisher, 19, engineer, Brighton; Clarence Smith, 18, student, Brighton; William H. Eccles, 18, apprentice druggist, Barrow-in-Furness; Frank G. Edmed, 17, student, Brighton; Henry T. Hildage, 19, fitter, Altrincham; Robert H. Watson, 24, student, Dublin; Harold Hibbert, 16, student, Manchester. Free Studentships—John W. Button, 23, tool fitter, Oldham; George H. Stanley, 17, student, London; Walter Eraut, 19, mechanical engineer's apprentice, London; I. William Chubb, 23, draughtsman, London; Robert H. H. Duncan, 15, student, Sunderland; George George, 18, student, Bristol. Percy

Nicholls, 23, engineer, of Pontefract, obtained the third place in order of merit in the competition for National Scholarships, &c., but withdrew before the awards were actually made.

THE separation of minerals of high specific gravity has recently been greatly facilitated by the introduction of the fused double nitrate of silver and thallium, originally due to Dr. J. W. Retgers. When these nitrates are brought together in the molecular proportion of 1 : 1, they yield a double salt, which fuses at 75° C. to a clear, mobile liquid, having a specific gravity of about 5 and miscible with water in all proportions at temperatures between its melting point and 100° C. The melting point also diminishes rapidly as water is added, going down to 50° or 60° C., and fusion and solubility pass uninterruptedly into one another. We have thus at our command for the separation of mineral particles a liquid far exceeding in specific gravity any of the previously described heavy solutions, and which has the advantage of being practically colourless, neutral, soluble in water, and of being readily recovered from the aqueous solution by simple evaporation on the water bath. Some further hints upon the use of this convenient medium are given in the current number of the *American Journal of Science*, by Messrs. Penfield and Kreider. Separations may be made in test tubes heated in a water bath. After a separation is completed and the fusion cooled, the test tube is broken and the solid cake divided, when the heavier and lighter portion may be obtained by dissolving the double salt. If fractional separations are required, the fused salt may be placed in a tube with a narrow neck at the bottom, into which is ground a glass rod to serve as a stopcock. This apparatus slips inside of a test tube to within a few millimetres of the bottom. The whole is heated in a beaker of hot water, and the liquid is stirred by means of a glass rod bent into a semicircle at the bottom. Heavy particles are drawn off by raising the ground glass rod. Small particles getting caught in the stopper can usually be ground out by twisting the rod, but in no case will such an accident cause great inconvenience.

THE annual report for 1893, just received from the Department of Mines and Agriculture, New South Wales, states that the Government metallurgical works will probably be started this year. These works in conjunction with the School of Mines which has been established at the Sydney University, enables the colony to offer as complete and effective a course of training in mining and metallurgy as can be obtained in Great Britain. The report consists largely of statistics relating to the output, value, &c., of various minerals. A boring made at Cremorne Point is of interest. Prof. T. W. E. David made some determinations of the temperature of this bore. The hole was 2929 feet deep, but the bottom 29 feet (about) was silted up with the powdered rock produced by the cutting action of the diamond drill used in the boring. From 2900 feet to within 300 feet of the surface, the bore was filled with water, the column being, therefore, 2600 feet high, and giving a maximum pressure of, approximately, a trifle over half a ton per square inch. The thermometers used were hermetically sealed in wrought-iron tubes and surrounded with brass filings and brass turnings. The readings obtained showed that the rock temperature at a depth of 2730 feet was 97°·5 F. The mean surface temperature at Sydney is about 63° F., so the rate of increase was 1° F. for about every 78 feet 10 inches.

THE *Bulletin* of the Royal Gardens, Kew, No. 92, for August 1894, is entirely occupied by a summary of information relating to Bananas and Plantains, with descriptions of the species and principal varieties of *Musa* grown for use and ornament.

FROM thirty to forty volumes are issued yearly in the comprehensive "Encyclopédie Scientifique des Aide-Mémoire" series published jointly by MM. Gauthier-Villars and M. Masson. The whole collection, when completed, will number three hundred volumes. The latest addition to the series is "Les Machines Thermiques," by Prof. Aimé Witz. The theory of steam, hot air, and gas engines is well described by the author, and the relations between different heat engines are set forth in a manner which brings out clearly the special characters of their respective cycles.

THE third volume of the *Seismological Journal of Japan*, corresponding to the *Transactions* of the Seismological Society, has reached us. Prof. John Milne, F.R.S., contributes to it a paper on "Seismic, Magnetic, and Electric Phenomena," in which he discusses the evidence as to the connection between those phenomena. Observations are adduced which seem to show that there may be a connection between earthquakes and magnetic and electric manifestations. But, concludes Prof. Milne, though "a variety of experiments and investigations have been made to test whether earthquakes were preceded, accompanied, or followed by magnetic or electric phenomena, the results obtained do not guarantee the existence of such connections. It does not seem likely that earthquakes can result from electric discharges, and it has not yet been proved that they give rise to electric phenomena. When they have resulted in the displacement of large masses of rocky strata, as happened in 1891 in Central Japan, slight local changes in magnetic curves have resulted, but beyond this and effects due to the mechanical shaking of earth-plates, our certain knowledge is exceedingly small."

THE "Geological Sketch-Map of Western Australia," by Mr. H. P. Woodward, the Government geologist, shows in a very clear manner the geology of the explored districts; but it also shows of how large an area the geology is quite unknown. The scale is 1 : 3,000,000 (1 inch to 47·3 miles). The rock-divisions coloured are: Recent and Tertiary, Mesozoic, Palæozoic, Metamorphic, Crystalline (schists and granite), Volcanic, and Plutonic (basalt and granite). The distribution is shown of gold, copper, lead, tin, and coal; gold and copper are indicated by solid colour, which somewhat interferes with the general effect of the map, as the colours appear to represent special formations; the other minerals are shown by coloured lines. The chief gold-fields are situated on metamorphic rocks; a few are in the crystalline areas. The geology of the Coolgardie and Dundas gold-fields is not indicated. The map is clearly printed, both in its topography and colouring. It is published, for the Government of Western Australia, by G. Philip and Son, Fleet Street, London.

THE *Monist* for July contains an interesting article by Mr. Wm. R. Thayer, on Leonardo de Vinci as a pioneer in science. Of the thousands of MS. pages which this indefatigable experimenter left, one volume alone has been edited. At first no one could decipher them, for Leonardo wrote backwards from right to left. The pioneer work in science, astronomy, physics, geology, botany—in fact, the whole circle of the sciences—contained in this one volume is briefly but clearly sketched, and some of his pithy and epigrammatic notes are quoted. "His curiosity was insatiable; his methods were observation and experiment; his advance was from the known to the unknown. . . . To search out all things, to experiment and verify, to let his eyes test, and reason be the judge. This was Leonardo's method." Prof. Hermann Schubert's article on "Monism in Arithmetic" is a lucid statement of fundamental principles in continuation of a previous paper on "The Notion and Definition of Number."

EXPERIMENTAL psychology is represented in *Mind* by an article on "Mediate Association," by Mr. W. G. Smith, the results of which do not appear to be very conclusive. In the *Psychological Review*, Prof. Ladd describes the results of experimental work on the "direct control of the retinal field." A class of sixteen students were asked to close the eyes, allow after-images to die away, and then to cause, by attentively willing, a cross, circle, or some other simple figure, to appear in the retinal field. In two cases, where the results were successful, a coloured figure was distinctly visualised, and when the eyes were opened after these voluntary crosses were obtained, and were immediately focussed on a sheet of white paper, a cross was found to appear on the paper in the complementary colour. It is clear that these experiments open up interesting psychological problems. Prof. Jastrow, in the same journal, describes experiments on Helen Kellar, a blind and deaf girl. The sensibility of her finger-tips and the palm of her hand were found to be decidedly more acute than in the average individual; her verbal memory is decidedly above the normal; and she shows that sensitiveness to vibration and jars which has frequently been noted in the deaf.

MR. THOMAS R. SIM, Curator of the Botanic Garden, King William's Town, South Africa, has done good service by collecting and systematically arranging the records of Kaffrarian plants, in a pamphlet recently published at Cape Town. As a botanical district, Kaffraria is described as an oblong tract of country two hundred miles long by about one hundred miles wide, bounded at one end by the Karoo, and at the other end by Natal. Mr. Sim finds that the flora includes 2449 species, of which 1690 are dicotyledons, 656 monocotyledons, and 103 vascular cryptogams. The richness in species is shown by a comparison with Great Britain—an area much greater than that of Kaffraria, but containing only about 1700 species. The opinion is expressed that were the Kaffrarian plants as well known as our own, they would number more than three thousand species. Though Mr. Sim's list is incomplete, it is an excellent groundwork upon which a detailed description of the flora of the district surveyed may be built.

FOR some years past Lieut.-General Pitt-Rivers has supplied the means for physical and mental recreation near his country seat at Rushmore. He has had the Larmer Grounds laid out as pleasure grounds, and opened them free to the public every day. In 1887 the number of persons who availed themselves of this privilege was 15,351, and last year the number was 24,143. To those interested in breeding and acclimatisation, some of the animals in the grounds at Rushmore will be found of interest. But the museum at Farnham, established and supported by General Pitt-Rivers, is most attractive to us. It consists of eight rooms and galleries devoted mainly to antiquities, and containing models of the excavations carried on by the generous donor in the neighbourhood. During last year, more than seven thousand persons visited the museum. Another building open is King John's House at Tollard Royal. This building contains a series of pictures illustrating the history of painting from the earliest times, commencing with Egyptian paintings of mummy heads of the twentieth and twenty-sixth dynasties, B.C. 1200–528. Descriptions of all these places are given in a short guide, recently received, together with illustrations of some of the most striking features.

STATISTICS relating to the distribution of rain over the British Isles have been collated by Mr. G. J. Symons, F.R.S., in "British Rainfall," for thirty-four consecutive years. The volume for 1893 resembles former issues so far as the tabular matter is concerned; but the great drought rendered the year an exceptional one in several respects. At twenty-five stations, only about an inch of rain fell from the end of February to the end of June, that is, during a period of four months. A curious point mentioned in connection with the discussion of this remarkably low rainfall is that among the stations recording droughts exceeding 120 days, two of the three which head the list were situated on promontories or projecting parts of the coast. These were Dungeness, with a period of 127 days, during which only 1·27 inches of rain were measured, and East Dean (near Beachy Head), where 1·18 inches fell in 126 days. Several remarkably heavy falls in short periods occurred during the year. At Preston, 1·25 inches is estimated to have fallen in five minutes on August 10; but this record is hardly trustworthy. A fall of 0·62 inch in five minutes was measured at Shirenewton Hall on June 14. This is at the rate of 7·44 inches per hour. An extraordinary fall of rain occurred at Eastbourne in July, and a waterspout (or cloud burst) caused great damage on the Cheviots in the same month. Various other remarkable falls of rain are recorded in the notes which the observers send to Mr. Symons with the results of their rain-gauge observations. A discussion of the relation of the total fall of rain in 1893 to the average shows that, taking the whole of the British Isles, the deficiency was 14 per cent.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Mr. H. P. Nicholls; an Ocelot (*Felis pardalis*) from South America, presented by Miss Edith Zambra; two Ring Ouzels (*Turdus torquatus*) from Hungary, presented by Mr. John Young; a Herring Gull (*Larus argentatus*), British, presented by Mr. George Hayes; a Common Viper (*Vipera berus*) from Gloucestershire, presented by Mr. Barry Burge.

OUR ASTRONOMICAL COLUMN.

SOLAR ELECTRICAL ENERGY.—According to Dr. M. A. Veeder (*Proceedings* of the Rochester Academy of Sciences, vol. ii., July 1894), there is conclusive evidence that magnetic perturbations are not of thermo-electric origin, and are not connected with heat and light radiations. He holds, in short, that there is no correspondence whatever between the behaviour of magnetic storms and the manner in which thermal and luminous radiations are originated and propagated from the sun to the earth. His idea is that electrical disturbances upon the sun are transmitted to the earth not by radiation but by conduction through the impalpable dust and débris with which interplanetary space is filled. Such meteoritic particles are composed of good conducting material, and an examination of a large number of meteorites has shown Dr. Veeder that they all possess magnetic properties which might have been produced by long-continued induction. Therefore he thinks that the origin of magnetic storms is as follows:—"Particular portions of the sun's surface and cooler immediate surroundings are electrified by what has every mark of being volcanic action. The motion of rotation of the sun carrying forward these charged portions of its surface, develops currents dynamically which act inductively along lines of force wherever there is conducting material within their scope. There is no conveyance by radiation or in a manner similar to that in which heat and light are conveyed from the sun. The laws governing the process are entirely different from those of radiation, and have reference to the principles of conduction as they appear under the conditions existing in interplanetary space. It is a mode of solar action that is distinct, and that must be considered by itself."

TEMPEL'S PERIODIC COMET.—This comet, rediscovered by Mr. Finlay at the Cape, on May 8, is still visible, and promises to be within the grasp of moderately large instruments for some time. M. Schulhof points out in the *Astronomische Nachrichten* that it is desirable that the comet should be followed as long as possible. The object is becoming more favourably situated

for observation, and there will be very little diminution in its light during the next three or four months. Using recent observations of the position of the comet, M. Schulhof has computed a new orbit. The following positions are extracted from the ephemeris based upon the new elements:—

Ephemeris for Paris Midnight.

1894.		R.A. h. m. s.		Decl. ° ′ ″
Aug. 27	...	3 53 39·6	...	+3 32 39
29	...	3 55 24·5	...	3 24 13
31	...	3 57 1·2	...	3 15 16
Sept. 2	...	3 58 29·6	...	3 5 51
4	...	3 59 49·6	...	2 55 59
6	...	4 1 1·1	...	2 45 41
8	...	4 2 3·9	...	2 35 0
10	...	4 2 58·1	...	2 23 56
12	...	4 3 43·3	...	2 12 31

A NEW VARIABLE STAR.—The Rev. T. E. Espin informs us, through a Wolsingham Observatory *Circular*, that the star DM + 50°·2251, the position of which is R.A. 16h. 1·5m., Decl. + 50° 47′, is variable.

ON THE NEWTONIAN CONSTANT OF GRAVITATION.[1]

III.

FIG. 8 is a view of the apparatus with the optical compass in position, and with the microscopes focussed upon the wires. They are then ready to be withdrawn by the focussing slide, so as to transfer the distances directly to the small glass scale, as already described.

When this is completed the proper windows are put in position, the screen tubes, the octagon house, and the felt screens are all placed ready for operation 10, in which the deflections are measured, and the period with the balls is determined. As this is the operation in which variations of temperature produce so serious an effect, I prefer to leave everything undisturbed for three days, to quiet down. A few hours are quite useless for the purpose.

In operation 11 the period with the counter-weight in the place of the gold balls is measured; also the deflection, if any, due to the lid and lead balls upon the mirror alone. This is only 1/10 division, but its existence is certain. In the later operations the deflections, if any, due to the lid alone on the mirror alone, and to the lid alone on the mirror and gold balls, are separately determined. Neither of these can be detected. The actual elongation of the fibre may also be observed at this stage, but this is of interest only as bearing on the elastic properties of quartz fibres under longitudinal strain.

Before I come to the treatment of the observations, I should like to refer shortly to the kind of perfection of conditions which by the employment of every practicable refinement that I could devise, I have succeeded in obtaining. Taking experiment 8 as an example, favourable in that the conditions were good, *i.e.* I was not badly disturbed by trains, wind, or earth tremors, I give the worst and the best sets of four points of rest obtained from six elongations. They were:—

Worst set + position	Best set − position
24491	20795·4
24493	20795·7
24493·5	20795·5
24492	20795·5
(24491·7)[2]	
24492·4 mean.	20795·5 mean.

Taking all the mean points of rest, as determined above, in groups of three to eliminate slow shifting, if any, of the points of rest, the series of deflections were:—

3696·0
3696·3
3696·0
3696·8

Continued from p. 363.

[2] Disturbed by trains.

(Interval of one hour, in which oscillations of large amplitude were observed for period.)

(3697·7)
3696·0

Immediately after the oscillations of large amplitude, which in this case at the end were rather badly disturbed by trains or otherwise, a rather different deflection was observed, but not seriously different. As examination of the figures shows only one anomalous point of rest immediately after the large amplitude disturbance, I feel justified in rejecting the only discordant figure, and in taking the mean of the rest as the true deflection. The unit in this case is 1/10 division. It corresponds to an angular movement of 1/280000, *i.e.* about three-quarters of a second of arc. Now a calculation of the angular twist due to a rotation of the air based upon the period, the moment of inertia, and the logarithmic decrement, shows that if the air in the tube were made to whirl round at the rate of one turn in six weeks, so that the air would blow past the gold balls at the rate of

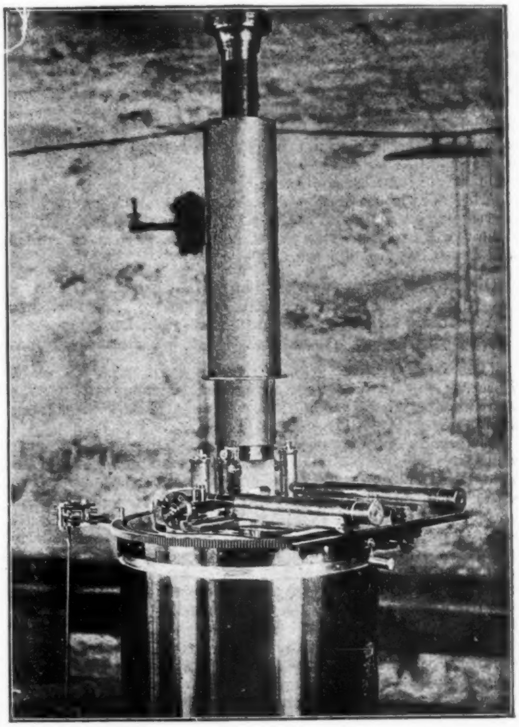

FIG. 8.

one inch in a fortnight, the deflection produced would be 1/10 division, an amount which is greater than the uncertainty of the deflection on this particular night. Again, an examination of the points of rest through the night in the positive and in the negative positions shows a very small steady creep, the same in each case. Creepage of this sort has been, I believe, mentioned as a defect of quartz fibres. When it gives trouble it is due to draughts, as already explained, or to imperfect attachment of the fibres.[1] In the present instance the creepage observed corresponds to a surface rate of movement on the fibre of a millionth of an inch a month.

An examination of the mobile system of the beam and suspended gold balls, of which I exhibit a greatly enlarged and working model, at once shows that all the parts are capable of independent movement to an apparently perplexing degree. This in the theory of the instrument I have

[1] See my paper on "Attachment of Quartz Fibres," *Phil. Mag.*, May 1894.

treated provisionally as a rigid system moving all as one piece, which it certainly does not seem to be. For instance, the lead balls, by their attraction of the gold balls, pull them out of the perpendicular, so that their distance from the axis is greater than that given by measurement by the optical compass. The error amounts, in the case of the lower ball, when the lead is at its nearest point, to 1/10,000,000 inch, and I have not taken any notice of it. When the beam is oscillating through so great an angle as 100,000 units the centrifugal force only makes the gold ball move out four times as much, and I have taken no notice of that. Again, when the mirror is under acceleration by the fibre, the gold balls, hanging 5 and 11 inches below, do not follow absolutely; they must lag behind, and so affect the period. Now in this case the gold balls are suspended in a manner which is dynamically equivalent to being at the end of a pendulum 5¾ miles long, the shortest equivalent pendulum that has ever been employed in work of this kind; but short as it is, I have not thought it worth while to be perturbed by an uncertainty of a few inches. There is one point which in some of the experiments only has amounted to a quantity which I do not like to ignore. It is due to the torsional mobility of the separate fibres, about which each gold ball hangs, allowing them in their rotation to slightly lag behind the mirror. As I did not see how to allow for it, I applied to Prof. Greenhill, who immediately told me what to do, and who, with Prof. Minchin, spent a day or two in the country, covering many sheets of paper with logarithms, in finding and solving for me the resulting cubic equation. The correction on this account is 1/7850 on the stiffness of the torsion fibre.

There are four remaining corrections depending on the fact that besides the gravitating spheres there are the ball-holders and supporting wires and fibres, all of which produce small but definite disturbances in the gravitation. These are all calculated and allowed for. They are:—

Disturbances due to brass-holders of lead balls...	1/7320
,, ,, copper ,, gold ,, ...	1/265,000
Attraction of lead balls for quartz fibres	+ 1/200,000
,, gold ,, phosphor-bronze wires	– 1/115,000

Then in experiment 9 gold cylinders were employed. Mr. Edser, of the Royal College of Science, calculated for me the correction to be applied if they were treated as spheres; this amounted to 1/3300.

I have already mentioned that experiment 8 was made under more than usually quiet conditions. Such extreme quiet is desirable, that I manage to reserve Sunday nights, from midnight to six or eight in the morning, for observations of deflection and period. All the other operations can be carried on in the daytime. Sunday is the only night that is suitable, as the railway companies spend every other night shunting and making up trains about a mile away, and this causes such a continuous clatter and vibration, that hours of work may be lost. A passing train does not seem so injurious; but, fortunately for me, most of the observations were made during the coal strike, and fewer trains than usual were running. However, though I may escape from the rattling traffic of St. Giles by working at night, and on Sunday nights am not so badly affected by the trains, I am still not sure of quiet even when there is no wind. For instance, at a quarter to four on Monday morning, Sept. 10, 1893, I was recording chronographically the passage of every ten divisions. Everything was quite quiet, and at the particular moment the marks on the drum recurred at intervals of about three seconds. Suddenly there was a violent non-vibrating lurch of fifteen divisions, or 150 units, which is enormously greater than anything that either trains or traffic could produce; of course, I could make no further record. The time of the last mark was, allowing for the known error of the clock, 15h. 44m. 14·3s. This was entered the same day in my note-book as an earthquake, and in Tuesday's *Standard* I read an account of a violent earthquake in Roumania at about the same time. I have not yet particulars from Vienna, for which Mr. Horace Darwin has written; but though the shock recorded in the newspaper seem to have been too late, preliminary shocks are by no means unknown, and I cannot help thinking that what I observed was the dying out and distant effect of one of these. Of this, however, I am sure, that it was an earthquake that I observed, and not any disturbance due to human origin.

Owing to the viscosity of the air, which limits the time during which an observation for period can be made to about 40 minutes, on account of the resistance that the slowly moving mirror and gold balls experience in their passage through it, I made one experiment, with the view of reducing this difficulty, by the use of an atmosphere of pure dry hydrogen gas, which possesses a viscosity only half that of air. I did find that on this account a great advantage could be gained; but this was more than counterbalanced by the difficulty of getting up a sufficient swing in the gas, and of efficiently controlling the mirror. At the same time, I think that if I had had time to provide means for feeding the gas into the tube without entering the corner, and at the same time were to prevent diffusion at the lower screw, that a little trouble in this direction would be well rewarded. Meantime I found within the limits of error, which were greater than without the hydrogen, that the deflection and the period corrected for the diminished damping were the same. The chief interest of this experiment lies in the fact that it revealed an action unknown to me, and I believe to others, that a thin plane glass mirror, silvered and lacquered on one side, definitely bends to a small extent, becoming slightly convex on the glass side when in hydrogen, and instantly recovers its form when surrounded by air again. This happened many times, producing a change of focus in the telescope of about five-eighths of an inch. I do not offer any explanation of the fact.

There is an observation which should be of interest to elasticians. In experiments 4 to 8 the torsion fibre carried the beam mirror and the ·25 inch gold balls, weighing, with their hooks and fibres, 5·312 grammes. In experiment 9, gold cylinders were substituted, weighing, with their hooks and fibres, 7·976 grammes. The weight of the mirror was ·844 gramme. In consequence of the small increase of load the torsional rigidity of the fibre fell more than 4 per cent., an amount far too great to be accounted for by the change of dimensions, even if Poisson's ratio were as great as ½. There is no doubt about the great reduction in stiffness, for this figure is one of the factors in the final expression for G, which does not show a change of more than 1 part in 1570.

It will not be possible at this late hour to explain how the observations are treated so as to obtain the value of G. It is sufficient to state that in one of these clips all the observed deflections and corrected periods are collected. In the second all the geometrical observations are collected and reduced, so as to obtain what I call the geometrical factor, *i.e.* a number which, when multiplied by the unknown G, gives the torsion on the fibre. In the third, the moments of inertia and periods are made use of to find the actual stiffeness of the fibre in the several experiments, and in the fourth these are combined so as to find G. From G the density of the earth Δ immediately follows.

The annexed table contains the important particulars of each experiment. From this it will be seen that the lead balls were twisted and interchanged in every way, so as to show any want of gravitational symmetry if it should exist. For instance, after experiment 7 the ball that was high was made low, the side that was outwards was turned inwards, and their distance apart was reduced by 1/50 inch, but the change in the result was only 1 part in 2764. The experiments 7, 8, 9, 10 were made under widely different circumstances. After experiment 8 the gold balls were changed for heavier gold cylinders, which, as has already been stated, reduced the torsion of the fibre by 4 per cent., but the result is practically the same as that of experiment 7. I then broke the end of the torsion fibre. After keeping it in London three months, I broke the other end. I then resoldered each end and put the fibre back in its place, and after making every observation afresh, found with the new shorter and stiffer fibre a result differing from that of experiment 8 by only 1 part in 27,635. These four experiments were all made under favourable circumstances, and on this account I feel more able to rely upon them than on the earlier ones, which were subject to greater uncertainty. The last experiment was made under most unfavourable conditions. The periods and deflections were taken in the first four hours after midnight, then, after a few hours' sleep, and far too soon for the temperature to have quieted down, I took the period with the counterweight, but was only able to give ten minutes, as I had to catch a train in order to be able to give my midday lecture at South Kensington. It is not surprising that under such conditions a difference of 1 part in 600 should arise. There is a difference of about the same order of magnitude between the earlier experiments and the favourable four. There

is one point about the figures that I should like to mention. No results were calculated till long after the completion of the last experiment. Had I known how the figures were coming out, it would have been impossible to have been biassed in taking the periods and deflections. Even the calculating boys could not have discovered whether the observed elongations were such as would give a definite point of rest. I made my observations, and the figures were copied at once in ink into the books, where afterwards they left my hands and were ground out by the calculating machine. The agreement, such as it is, between my results is therefore in no way the effect of bias, for I had no notion till last May what they would be.

No. of Exp.	Lead balls: Arch side	Lead balls: Wall side	Lead balls: Shellac spots	Gold balls: Arch side	Gold balls: Wall side	Neutral lid reading	Date	Deflection	Geometrical factor	Stiffness of fibre.	Result: G	Result: Δ
				1·3 grammes each			1892			·00	·0000000	
3	2 low	1 high	Inwards	4 low	3 high	267	Oct. 1–30,	5637·3	6089·89	245483	66645	5·5213
				Gold balls of double weight			1893					
4	2 low	1 high	Inwards	4 low	3 high	267	Aug. 15–Sept. 3				66702	5·5167
5	1 high	2 low	Inwards	3 high	4 low	86·5	Sept. 4–11	3667·6	12423·8	772200	66711	5·5159
6	2 low	1 high	Inwards	4 low	3 high	265·9	Sept. 12–14	3667·7	12422·3	Same as No. 8	66675	5·5189
7	2 low	1 high	Outwards	4 low	3 high	265·9	Sept. 15	3664·0	12432·8	Same as No. 8	66551	5·5291
8	1 low	2 high	Inwards	4 low	3 high	265·9	Sept. 16–18	3695·2	12534·2	771664	66575	5·5271
				Gold cylinders								
9	1 low	2 high	Inwards	3 low	1 high	86	Sept. 27–Oct. 3	5775·5	18800·5	739988	66533	5·5306
							1894					
10	1 low	2 high	Inwards	4 low	3 high	85·25	Jan. 1–13	3515·4	12531·8	811011	66578	5·5269
11	1 low	2 high	Inwards	4 low	3 high	85·25	Jan. 14		Hydrogen experiment			
12	2 high	1 low	Inwards	3 high	4 low	265·2	Jan. 17–21	3520·5	12533·7	811385	66695	5·5172
									Adopted result ...		66576	5·5270

My conclusion is that the force with which two spheres weighing a gramme each, with their centres 1 centimetre apart, attract one another, is $6·6576 \times 10^{-8}$ dynes, and that the mean density of the earth is 5·5270 times that of water.

It is evident, from what I have already said, that this work is of more than one-man power. Of necessity I am under obligations in many quarters. In the first place, the Department of Science and Art have made it possible for me to carry out the experiment by enabling me to make use of apparatus of my own design. This belongs to the Science Museum, where I hope in time to set it up so that visitors who are interested may observe for themselves the gravitational attraction between small masses. Prof. Clifton, as I have already stated, has given me undisturbed possession of his best observing room, his only good underground room, for the last four years. The late Prof. Pritchard lent me an astronomical clock. Prof. Viriamu Jones enabled me to calibrate the small glass scale on his Whitworth measuring machine; and Mr. Chaney did the same for my weights. I would especially refer to the pains that were taken by Mr. Pye, of the Cambridge Scientific Instrument Company, to carry out every detail as I wished it, and to the highly skilled work of Mr. Colebrook, to which I have already referred. Finally, I am under great obligations to Mr. Starling, of the Royal College of Science, who performed the necessarily tedious calculations.

In conclusion, I have only to say that while I have during the last five years steadily and persistently pursued this one object with the fixed determination to carry it through at any cost, in spite of any opposition of circumstance, knowing that by my discovery of the value of the quartz fibre, and my development of the design of this apparatus, I had, for the first time, made it possible to obtain the value of Newton's Constant with a degree of accuracy as great as that with which electrical and magnetic units are known, though I have up to the present succeeded to an extent which is greater, I believe, than was expected of me, I am not yet entirely satisfied. I hope to make one more effort this autumn, but the conditions under which I have to work are too difficult; I cannot make the prolonged series of experiments in a spot remote from railways or human disturbance; I cannot escape from that perpetual command to come back to my work in London; so I must then leave it, feeling sure that the next step can only be made by my methods, but by some one more blest in this world than myself.

SCIENCE IN THE MAGAZINES.

IN the August magazines received by us, science is but poorly represented. A brief mention of the more important articles will therefore be sufficient this month.

Mr. Benjamin Kidd's work on "Social Evolution" has furnished material for much criticism. In the *National Review* Mr. Francis Galton, F.R.S., discusses the part of religion in human evolution as set down in the book; and Mr. Kidd adds a short note on the opinions expressed in the article. The same magazine contains a paper on "Sleeplessness" by Mr. A. Symons Eccles, and one on "Colliery Explosions and Coal Dust," by Mr. W. N. Atkinson. An experience of fifteen years in investigating explosions in coal-mines has led him to believe that "coal dust has been the chief, or only, agent in all recent widespread colliery explosions." It is regretted that "no experiments have been made on a scale large enough to yield visual demonstration of the effect of an explosion of coal-dust, under conditions approximating as closely as possible to those existing in mines. The nearest approach to such experiments in this country were those recently made by Mr. H. Hall, H.M. Inspector of Mines, in an old pit shaft fifty yards deep. The length of such a shaft is insufficient to develop the whole force of a coal-dust explosion, and the conditions under which the explosions or ignitions took place were necessarily different from those obtaining in the practical working of mines. These experiments, however, are valuable, as demonstrating that the dust ordinarily existing in a great number of mines (not particular exceptional coal-dusts) are capable of propagating flame to the full limits admitted by the conditions of the experiments."

A psychological paper, entitled "How We Think of Tones and Music," is contributed to the *Contemporary* by Mr. R. Wallaschek. Mr. Andrew Lang tilts at Prof. Huxley's treatment of the Bible story of Saul and the Witch of Endor "as a piece of evidence bearing on an important anthropological problem," and treats the matter from a less scientific point of view.

Eight recent books on Iceland furnish the subject of an interesting account of the island in the *Quarterly Review* (No. 357). The same publication contains a long article on "Forestry," in the course of which the author says that the three great faults noticeable in the treatment of woods in Great Britain are: (1) Discrimination has seldom been shown with regard to the choice of the kinds of trees for given soils and

situations. (2) Plantations have not usually been formed of the best degree of density for the given kinds of trees selected for planting. (3) A sufficient density of crop has not always been maintained during the subsequent periods of the natural development of the trees. It is finally concluded that—"Better results than can at present be reasonably expected would probably be obtained if State aid were freely granted towards the dissemination of sound instruction concerning sylviculture; and the only proper places for bringing this within the reach of the future landowners, and of young men of good education, are undoubtedly the great Universities."

Messrs. T. G. Allen and W. L. Sachtleben continue, in the *Century*, the description of their journey "Across Asia on a Bicycle," from Constantinople to Pekin. Dr. W. T. G. Morton's claims to the discovery of anæsthesia are championed by Mr. E. L. Snell. It will be remembered by readers of this monthly summary that the January number of the *Century* contained an article in which Miss E. B. Simpson told the story of her distinguished father's discovery of the anæsthetic properties of chloroform in 1847. It is now shown that, in the preceding year, Dr. Morton publicly demonstrated the use of sulphuric ether in producing anæsthesia, at the Massachusetts General Hospital.

A passing mention will suffice for the remaining articles of scientific interest in the current magazines. Mr. A. H. Savage-Landor describes a visit to Corea, in the *Fortnightly*. Some of the possibilities of the phonograph are foreshadowed in *Scribner*, by Octave Usanne. *Longman's Magazine* contains "White Sea Letters, 1893," by Mr. A. Trevor-Battye. Naturalists will find the letters interesting. Under the title "Land Crabs," Mr. E. Step contributes to *Good Words* a popular description of such terrestrial crustaceans as *Gecarcinus ruricola* and various members of the genus *Gelasimus*. Finally, the vivisection controversy is continued in the *Humanitarian*, and the man-like apes in the Gardens of the Zoological Society are described in the *English Illustrated*.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 21.—"Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. Part IX. Section 4. On the Gomphodontia." By H. G. Seeley, F.R.S.

"On an Instrument for Indicating and Measuring Difference of Phase between E.M.F. and Current in any Alternating Current System." By Major P. Cardew, R.E.

"On the Difference of Potential that may be established at the Surface of the Ground immediately above and at various Distances from a Buried Mass of Metal Charge from a High Pressure Electric Light Supply." By Major Cardew, R.E., and Major Bagnold, R.E.

PARIS.

Academy of Sciences, August 13.—M. Lœwy in the chair.—The death of M. Gustave Cotteau (August 10) was announced; the deceased correspondent was an authority on sea-urchins.—Note on the long-period meteorograph to be placed in Mont Blanc Observatory, by M. J. Janssen. A description is given of the arrangements for automatically registering during eight months the indications of the barometer, thermometer, and hygrometer, and the speed and direction of the wind.—New researches on the infra-red region of the solar spectrum, by M. Langley. The author shows that a perfected arrangement of the bolometer is able, by means of automatically photographing the movements of the galvanometer needle, to furnish a complete record in an hour of the infra-red region of the solar spectrum with very great accuracy. The accuracy obtained is illustrated by the case of the D lines in the visible part of the spectrum; the method indicates very clearly the Ni line occurring between D_1 and D_2. Crystals collect at the upper part of a less dense solution, by M. Lecoq de Boisbaudran. If a saturated solution of carbonate and thiosulphate of sodium be saturated with sodium sulphide ($Na_2S.9H_2O$) and a quantity of the latter placed at the bottom of the solution and a small fragment supported near the surface, in the course of a few days or weeks the additional sodium sulphide is found collected round the fragment on the support. By the solution of the crystallised sulphide the bulk of the solution increases in a greater ratio than the weight, and hence its specific gravity is lowered.—A new use of Plücker's conoid, by M. A. Mannheim.—New arithmetical theorems, by Père Pepin.—Remarks on the electrochemical graphic method of studying alternating currents, by M. A. Blondel.—Application of auto-collimation to the measurement of indices of refraction, by M. Féry.—On the specific heat of liquid sulphurous anhydride, by M. E. Mathias. A general method is described. The true specific heat of liquid sulphur dioxide is always positive and increases constantly and indefinitely with the temperature. A table is given showing the value of m between $-20°$ and $+155·5°$. Between $-20°$ and $+130°$, $m = 0·31712 + 0·0003507t + 0·0000067 62t^2$. At $155·5°$, $m = 2·980$.—On benzoylquinine, by M. A. Wunsch. The base has been obtained in clear, colourless prisms, insoluble in water. It dissolves easily in alcohol, benzene, chloroform, petroleum ether, carbon bisulphide, and ether. It has the composition $C_{20}H_{23}(C_6H_5CO)N_2O_2$, and melts at 139° without decomposition. The following salts have been examined: the basic and normal hydrochlorides, and the basic salicylate, tartrate, and succinate.—On the heart in some orthoptera, by M. A. Kowalevsky.—On the perithecæ of the vine mildew (*Uncinula spiralis*), by M. Pierre Viala. The abundance of perithecæ found in 1893 fully confirms the identity of *Erysiphe Tuckeri* with *Uncinula spiralis*. The parasite noted by Bary on the conidiophores of mildew, *Cicinnobolus Cesatii*, was abundantly developed in 1893 in the perithecæ of *Uncinula spiralis*. The author also describes a peculiar parasitic bacterium.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Alembic Club Reprints, No. 7—The Discovery of Oxygen, Part 1; Dr. J. Priestley (Edinburgh, Clay).—Annual Report of the Department of Mines and Agriculture, New South Wales, 1893 (Sydney, Potter).—Hygiene: Dr. J. L. Notter and R. H. Firth (Longmans).

PAMPHLETS.—Ueber den Osmotischen Druck von Lösungen von endlicher Konzentration: T. Ewan (Leipzig).—The Glaciation of the West of Scotland: D. Bell (Glasgow).—On the Selection of suitable Instruments for Photographing the Solar Corona during Total Solar Eclipses: A. Taylor (Dublin).—Les Grands Instruments de L'Avenir par M. Alvan Clark et la Fabrication des Grands Objectifs d'Astronomie par M. Mantois (Paris).

SERIALS.—Journal of the Franklin Institute, August (Philadelphia).—Michigan State Agricultural College Experiment Station, Horticultural Department, Bulletin 3 (Michigan).—Journal of the College of Science, Imperial University, Japan, Vol. 6, Part 4, and Vol. 7, Part 1 (Tōkyō).—Mittheilungen der Deutschen Gesellschaft für Natur und Völkerkunde Ostasiens in Tokio, Supplement, Heft 1, zu Band 6 (Tokio).—Royal Natural History, Part 10 (Warne).—Journal de Physique, August (Paris).—L'Anthropologie, tome 5, No. 4 (Paris).

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